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TRANSACTIONS

OF THE

AMERICAN INSTITUTE

OF

ELECTRICAL ENGINEERS.



CONTENTS.

FOURTEENTH GENERAL MEETING.

Greenacre-on-Piscataqua, Elliot, Me., July 26th to 28th, 1897.

	PAGE.
Associate Members Elected and Transferred.....	405
Steinmetz on Induction Motors. Discussion July 26th, 1897.....	407.
Thomson on New Induction Coll. Discussion July 26th, 1897.....	413
Welcoming Address, Dr. Geo. F. Barker. July 26th, 1897...	414
Macfarlane on Discharge of Condenser. Discussion July 27th, 1897.....	419
Bates and Barnes on Insulating Material. Discussion July 27th, 1897.....	429
Goldsborough on Armature Inductance. Discussion July 27th, 1897.....	439
Haskins on Electric Metering Discussion July 27th, 1897.....	443
Bosch on Fire Alarm Telegraph. Discussion July 27th, 1897.....	451
Gerry on Electric Traction Discussion July 27th, 1897.....	453
Foster on Cost of Steam Power. Discussion July 28th, 1897.....	461
Freedman, Burroughs and Rapaport on Enclosed Arc Lamps. Discussion July 28th, 1897.....	470
Owens, Hawksworth and Doubrava on Rotary Converter. Discussion July 28th, 1897.....	479
Jackson on Electrical Cooking. Discussion July 28th, 1897.....	491

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AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

FOURTEENTH GENERAL MEETING.

GREENACRE-ON-THE-PISCATAQUA,
ELIOT, ME., July 26, 27 and 28, 1897.

The opening session of the Fourteenth General Meeting of the INSTITUTE was called to order by President Crocker at the "Eirenion," Greenacre, on Monday, July 26th at 2 P. M.

The Secretary announced that the papers to be presented had already been printed in the May, June and July issues of the TRANSACTIONS with the exception of the last three appearing on the programme.

The following Associate Members were elected and transferred at the meeting of Council, June 23, 1897.

Name.	Address.	Endorsed by.
ATKINS, HAROLD B.	Engineer, A. K. Warren & Co., 451 Greenwich St., N. Y. City, residence, Roselle, N. J.	Townsend Wolcott O. O. Mailloux. A. K. Warren.
COPELAND, CLEMENT A.	Manager of Electric Lighting Station and Electrician, Copper Queen Consolidated Mining Co., Bisbee, Arizona.	Edw. L. Nichols. Fred'k. Bedell. Harris J. Ryan.
DAMON, GEO. B.	Consulting Engineer, F. P. Sheldon & Co., 612 Exchange Building, Boston, residence Lowell, Mass.	C. F. Bancroft. Chas. A. Stone. Russell Robb.
EDWARDS, I. R.	Construction Engineer, General Electric Co., 315 Buffalo Ave., Niagara Falls, N. Y.	Chas. P. Steinmetz Ernst J. Berg. Paul M. Lincoln.
ELLAARD, JOHN W.	Treasurer, Edison Electric Illuminating Co., 15 South Street, Baltimore, Md.	J. F. Morrison. Chas. F. Wallace. H. K. McCay.
SCOTT, WM. M.	Electrical Engineer. The Cutter Electrical and Mfg. Co., 1112 Samson St., Philadelphia, Pa.; residence, 108 West Johnson St., Germantown, Pa.	Clayton W. Pike. W. E. Harrington. H. F. Albright.

WILLIAMS, ARTHUR	General Inspector, The Edison Electric Illuminating Co., of New York; residence, 155 Linden Boulevard, Brooklyn, N. Y.	J. W. Lieb, Jr. A. E. Kennelly. Townsend Wolcott Douglas Burnett.
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Total 7.

TRANSFERRED FROM ASSOCIATE TO FULL MEMBERSHIP.

Approved by Board of Examiners June 21, 1897.

HAMMER, E. W.	New York City.
LEWIS, MINFORD,	Philadelphia, Pa.
Total 2.	

Mr. Charles P. Steinmetz then read the following paper on
"The Alternating Current Induction Motor."

[See May issue of TRANSACTIONS p. 175.]

DISCUSSION.

THE PRESIDENT:—We are greatly indebted to Mr. Steinmetz for giving us these very definite and complete figures. The induction motor is a new piece of electric mechanism, and data, particularly such data as this, concerning it, are very valuable. He not only considers the ordinary type of motor, but also, the effect on the efficiency, torque, speed, etc., when the various factors such as resistance, impedance, etc., are exaggerated. The paper is open for discussion.

PROF. ELIHU THOMSON:—Mr. Steinmetz has given a vast amount of study to the problem of induction motors. I can say that he has done a good thing in giving the results of the work to the world, so that it may serve as a guide to others working on this general subject. The paper is full of material which requires a good deal of time and study to discuss, and I look forward to the time when in these other promised papers we shall see the rest of the matter presented. The subject is likely to be an exhaustless one, as the number of changes which can be rung upon this type of apparatus and similar types is almost endless. I simply call attention to what seems to me quite an interesting matter, which would be of course naturally expected, that is comparing Fig. 3 with Fig. 9, one machine run as a motor simply, and the other as an induction generator; we see the speed curves are almost the exact counterpart reflected upward, while the other curves are very much the same, and the breakdown line is marked in very much the same way, by a re-curving backward at a certain elevation above, and below in the other instance. No doubt further inspection of these very interesting curves would discover other analogies between the work of the different motors and under the different conditions.

THE PRESIDENT:—The use of an induction motor as a generator is a very interesting subject, about which there is not very much information now available. Mr. Steinmetz simply considers it incidentally in order to complete the scope of his investigations. The stability of their action is very peculiar, and he does not specify the exact conditions, but indicates them. I hope that point will be more fully covered in future papers.

PROF. W. E. GOLDSBOROUGH:—It seems to me that one of the most interesting points which the paper brings out is the great exactness with which a piece of apparatus can be designed. The curves which are shown in this paper, especially those of Figs. 1 and 2 are particularly valuable to us, in view of the fact that they indicate that a piece of electrical apparatus can be designed with as great, if not greater, accuracy than can any other kind of machinery. The points fall upon the curves with great exactness, and if I understand Mr. Steinmetz aright, the curves were figured out before the machine was operated. I do not know of any other case where the accuracy of the calculations made previous to the test of the machine has been anything like as marked

as in the case we have before us, and this agreement between theory and practice seems to be something that is worthy being emphasized.

DR. A. E. KENNELLY:—I think this paper is both interesting and important. It shows that the quantitative behavior of the induction motor depends on two vector quantities; so that, when these vectors are known, all the behavior of the motor can be predicated.

I am desirous of clearing up two points in the paper that seem to be open to misapprehension. On page 183 it is stated that the characteristic features of the motor depend merely upon its admittance and impedance. But unless otherwise defined, the admittance and impedance of a motor or any conducting circuit should be mutually reciprocal quantities, whereas this admittance and impedance are not apparently so connected, and it appears, in fact, that the impedance is exclusive of the mutual inductance of the machine, while the admittance includes it. It seems important to have this distinction pointed out.

As regards the term "torque-efficiency," the definition given in words on page 180, seems to be excellent and one much needed: namely, the ratio of the torque which a motor develops at a given intake to the torque it would develop if it had no losses of energy. The symbolic definition for the torque efficiency on page 179, I do not understand, because it is expressed as a ratio of a torque to a power. It is true that torque and power have the same dimensional formula in our existing system of dimensions, but, we are accustomed to define an efficiency as the ratio of two quantities of the same nature, and not of two quantities of different natures. I think Mr. Steinmetz will assist us by clearing up this point.

DR. F. A. C. PERRINE:—I do not know whether I misunderstood Mr. Steinmetz or not, in reference to Fig. 13, page 201. If I did not, I cannot see how his statement agrees with the figure. It is stated that in spite of the fact that by the introduction of capacity the torque was tremendously increased, at the same time the torque efficiency was so largely decreased, that the effect of capacity was rather harmful than beneficial, and that instead of introducing capacity in order to get a high torque we should raise the voltage. The apparent torque efficiency certainly has a maximum coincident with the maximum of torque where the capacity reactance is introduced, though the torque efficiency is somewhat lowered. But I fail to see how the difference is so great as to warrant the statement that it would be better to increase the electromotive force in order to get the high current necessary to give the great torque, than to introduce the capacity to get that torque.

Then in reference to Figs. 1 and 2, as Prof. Goldsborough says, they are of tremendous importance, as showing how close our calculations can come to simple experimental results. At the

same time I do not understand from the paper how many of the initial points in these various curves are necessary to assume before the method of calculation can be applied and the curves drawn. For if initial points must be assumed for all these curves, then that means simply that the form of the curve is shown by the equation, but it does not show that a predetermination of the characteristics of such a machine may be obtained. From a comparison of Figs. 3 and 9 it is apparent that only the initial value of the current curve is assumed in these various calculations; but it is not at all clear that it is not necessary to assume more than the initial values of the other curves as well, and the equations simply give the forms of the curves. Those are points of difficulty to me that I would be glad if Mr. Steinmetz would give me further light on.

MR. CHARLES P. STEINMETZ:—Referring first to the effect of capacity inserted in the secondary of an induction motor, on page 201 a curve is shown giving the effect of various values of capacity introduced into the secondary, without any additional resistance. From this curve it follows that the maximum value of apparent torque efficiency is reached at the maximum point of torque, that is at the point of complete resonance. But this maximum value of apparent torque efficiency is only 51 per cent. and on either side of this point the apparent torque efficiency falls off very rapidly. Looking now on the preceding page, the curve of apparent torque efficiency produced by various values of non-inductive resistance inserted into the induction motor secondary, we see that the value is beyond 80 per cent. over a very large range, and hardly anywhere, except very near the short circuit point, drops below 80 per cent. This means that we can get a very large torque indeed by capacity, but at the expense of a still larger current, while by means of non-inductive resistance, and by raising the impressed voltage, we can get the same large torque with much less current. The best value of capacity is indeed the value giving perfect resonance, but even at that value the apparent torque efficiency is far below the values available by the use of non-inductive resistance.

With regard to the assumptions made in calculating the curves given in the paper, as stated therein, these curves are calculated from the values of impedance and admittance, both vector quantities. No further assumptions have been made, neither the initial point of the curve nor any other point. Obviously the initial point of the induction motor curve and of the induction generator curve are the same, since they are identical, representing the point of synchronism. These values of impedance and admittance from which the curves in the paper are calculated, may either be taken from tests made on the motor, or they are calculated beforehand in the same way as electrical data are generally calculated. The methods of calculation obviously belong in the field of electrical design and thus are outside of the scope

of this paper. I may say, however, that these values, and therefrom the induction motor curves, can be calculated with perfect accuracy, and in fact have to be calculated very accurately, since obviously in larger motors it is generally not possible to change them after the motor has been built, and thus they have to be calculated correctly before the motor is built.

The formula for torque efficiency and apparent torque efficiency, $\frac{T}{P}$ and $\frac{T}{Q}$, is apparently the ratio of a torque over a power. However, in discussing torque of a motor, the same difficulty appears already which has led me to the introduction of the terms "torque efficiency" and "apparent torque efficiency." The value of torque in pounds at one foot radius has obviously no meaning whatever in regard to the electrical features of the motor, since a motor giving only half the torque of another motor but being designed for twice the speed of operation is obviously just as efficient regarding the torque produced by it, or inversely, a motor with a larger torque when designed for lower speed of operation may be inferior in starting efficiency to a motor of same capacity of lesser starting torque but higher running speed. That means torque, and more particularly starting torque can be compared only with reference to the speed for which the motor is designed. Since in the discussion of the paper no assumptions are made with reference to the number of poles and the frequency of the motor, that is to its speed, the torque in pounds at one foot radius cannot be calculated at all without these assumptions, but only the product of the torque into synchronous speed, which gives a direct criterion of the starting effect of the motor.

It is this value which I have introduced into the paper as T and spoken of as torque. It is the torque, but with a multiplier depending upon the construction of the individual motor and its frequency, and this multiplier is its synchronous speed. Thus T is in reality a power, and is given in watts and represents thus the power which the torque of the motor would develop at synchronous speed. Hence the name synchronous watts. This I believe will make the apparent discrepancy in the paper disappear.

The terms admittance and impedance introduced in the paper need some further explanation I conceive, but have been left without it, since they have been used in the same meaning by me before, in the theory of induction motors and of transformers.

Admittance, however, is usually spoken of as primary admittance and refers to the admittance of the primary circuit only, at open secondary circuit.

If an *e. m. f.* is impressed upon an induction motor or transformer, a magnetic flux is produced thereby which is partly interlinked with the primary and secondary, partly interlinked only with the primary circuit, or only with the secondary circuit. The former is the flux of mutual induction, the latter the flux of self-induction. The effect of these two magnetic

fluxes is entirely different, and even opposite, and thus it is not well possible to investigate the action of an induction motor or transformer without separating these two fluxes or their corresponding *E. M. F.*s. This separation is done in introducing two different quantities, the primary admittance and the impedance. The primary admittance refers to the flux of mutual induction only, that is, the magnetic flux produced by the primary and interlinked with the secondary; while the impedance, or more properly self-induction impedance, refers to the flux of self-induction, or flux interlinked with one circuit only.

Obviously the investigation could be carried through by using either only impedance or only admittance for both quantities, but here in the theory of the induction motor as of the transformer, it is more convenient to represent mutual induction by an admittance and self-induction by an impedance.

The induction generator is a very important piece of apparatus, I believe. It is, however, somewhat restricted in its application, due to the necessity of having as load a circuit of leading current. But wherever the conditions are such that it can be used, as for operating synchronous motors or rotary converters, this type of machine has the great advantage of the absolute absence of continuous current exciting circuits, collector rings, or any other parts requiring attention. The voltage is generated in a stationary structure and the revolving part is a solid structure of iron and copper bars. Furthermore, as soon as the circuit is opened or short-circuited, the power is gone and the machine dead, so that you get here a type of alternator requiring no attention whatever. Besides you can run it at different speeds and still get the same frequency out of it.

There is another interesting feature noticeable when comparing the induction motor curves and the induction generator curves. The same machine as induction generator gives a considerably larger output electrically than as induction motor mechanically. In a future paper I shall dwell more particularly on the induction generator, and may mention here only that I have operated synchronous motors from an induction generator; and with the same voltage at the terminals of the induction generator, the mechanical output from the synchronous motor driven by the induction generator was larger than the maximum mechanical output which could be derived from the same induction machine as induction motor.

With regard to the agreement of tests with calculation, the agreement in induction motors is usually much closer than it is in continuous current machinery, and when you look into it you will see the reason for it. In continuous current machinery the magnetic circuit is worked at or beyond the point of the magnetic characteristic where saturation begins, and thus the *M. M. F.* consumed in the iron part of the magnetic circuit is a noticeable part of the whole *M. M. F.* Now it is not possible to predetermine the

magnetic characteristics of the material of a machine with absolute accuracy, since even a test made on a test piece broken from the casting of the very machine does not give correct results, different parts of the same casting differing from each other. In induction motors, however, and transformers the iron is necessarily working at a density below saturation, and thus the *M. M. F.* consumed in the iron is very small. Hence the main source of discrepancy between calculation and test in continuous current machines, does not exist in induction motors. Obviously occasionally some discrepancy between test and calculation appears in induction motors also.

The term admittance essentially depends upon the length of the air-gap of the motor. The length of air-gap used in induction motors is very small, and thus a variation in the air-gap due to imperfection of mechanical construction to a certain extent unavoidable. Occasionally I have found a discrepancy between the calculated and observed value of impedance and especially its energy component, the effective resistance, that observed by the test being larger than the ohmic resistance of the motor as derived from calculation or measurement. In most cases of this nature I have been able to locate the discrepancy in eddy currents produced somewhere in the mechanical structure, due to the proximity of iron or other solid metal, and by removing it have seen the test curve drop back to the curves derived from calculation. In the primary admittance of motors with very small air-gap necessarily a certain variation must be allowed for the mechanical impossibility of getting the length of a very small air-gap exactly right. Otherwise, however, you see that the problem of predetermination of induction motors is really more favorable for accurate solution than that of continuous current machines.

I believe this is all which has come up in the discussion.

The following paper on a "New Form of Induction Coil" was then presented by Prof. Elihu Thomson.

[See page 209, May issue.]

DISCUSSION.

DR. A. E. KENNELLY:—I think that this is an ingenious device and by giving my interpretation of its use, I may, perhaps, not only show some of its advantages over other forms, but also elicit Prof. Thomson's criticisms, in case I fail to describe the apparatus adequately.

When you excite the primary winding of an induction coil through an interrupter or revolving contact-breaker from a 110-volt circuit, it is difficult to break the circuit at the interrupter promptly and to avoid a vicious spark. If, however, you use a coarse-wire primary winding and a storage battery or low-tension source of excitation, the sparks and breaks are much less troublesome.

Prof. Thomson says, I want to use a 110-volt primary exciting circuit for convenience, and yet obviate the difficulty of breaking the arc at the interrupter. I do this by short-circuiting an intermediate coarse-wire coil just before each interruption, and I thus start a powerful low tension current in the intermediate coil with the energy which would give rise to the primary spark. Now the primary current can be interrupted without difficulty. As soon as it is safely broken, I break the powerful current in the intermediate circuit, just as though it were obtained from a low-tension source and obtain all the effects in the secondary circuit due to a low-tension excitation.

PROF. THOMSON:—If there are no further remarks I would say that Dr. Kennelly is quite right. He has put the case exactly as I would have had it put. The use of 110 volts in working the induction coil has required hitherto a very high-speed break and only moderate currents flowing in the coils, in which case there is a tendency to get in the secondary, alternating discharges or discharges very similar to high-frequency discharges, positive and negative. But in this case the tendency is to a unidirectional discharge as in the ordinary Ruhmkorff. At the same time I should have said that the working of the break-piece in this case is substantially without spark, and can be worked without any such devices as air-blast and without working under water. It can even be oiled while it is in operation and the effect is perfect. The objection to the arrangement of course is, that it involves the use of a revolving break-piece to put the circuits through these various paces; that is the connections must be made and broken in just a certain order and with just a certain relation to each other, that has to be determined beforehand and settled once for all. It is not of course as simple as a simple hammer break which opens a contact, but it is certainly more simple than a revolving break which has to have an air-blast or arrangement for continuously putting out the spark. Even then one gets a good deal of fire at the contacts.

MONDAY, JULY 26th, 1897.

EVENING SESSION.

The formal opening exercises of the 14th General Meeting were held in the Eirenon at 8.15 P. M. where the INSTITUTE was given a hearty welcome by Miss Sarah J. Farmer, and her representative, Dr. George F. Barker in the following words:—

MISS FARMER:—Mr. President, members of the INSTITUTE and ladies. The loving-kindness which you have shown in coming to us, participating in our conference and assisting in our loan exhibit touches me deeply. I feel more than I can express to you, and I have asked my father's friend who loved him, and whom my father loved, Dr. George F. Barker of the University of Pennsylvania to speak the greetings for me to-night.

DR. BARKER:—I have enjoyed, ladies and gentlemen, the atmosphere of Greenacre in the few hours that I have spent here, but there seems to me to be in the atmosphere an element that is peculiar. I have no physical reason for believing that ideas are mixed in the atmosphere as material substances are, but I imagine they are. At all events I find myself to-night inclined to think that there is a peculiar atmosphere here which I have not seen put down in the programme. I feel as though I had been hypnotized, and I am not sure but that it is so, for the hypnotists tell us that the power of a stronger mind over a weaker, forces the weaker to do whatsoever the stronger commands. Those of you who have been here these past weeks and those of you who have been here for the past years know well what I mean. A mind which can organize such an institution as you have here, a mind which can bring all these people together, full of ideas to give, and to receive from each other, is a stronger mind, and I am delighted myself to be put hypnotically under the influence of a stronger mind which I am sure we all agree Miss Farmer is. I justify my remarks then by simply saying that I am speaking for her and when I offer words of welcome I do it under hypnotic influence and I am speaking for her.

We all have some ideas about a future state, about immortality; but howsoever we differ in regard to what that actually means, we all agree in this, that the immortality of this life is quite as important and quite as powerful as the immortality of the future life. He who does for his generation that which is worthy of being done; he who improves the condition of his fellow men; he who invents for their benefit; he who discovers that they may live better and be better, is sure of a present immortality. Now to what I am asked to welcome the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS? To this atmosphere, an atmosphere sacred to the memory of Moses G. Farmer. It might be to this beautiful place, it might be to all the attractions which are here, it might be to all the delightful people whom we are to meet, but those

are as nothing in comparison with the magnificent atmosphere of the doings of Mr. Farmer, which they find here and which they breathe. We can breathe in an inspiration as we can breathe in the atmosphere, and none of us who have been here and paid our homage to the memory of that great man will fail to go away from here feeling that the inspiration of a great name has been more to us than all else which is here beside.

Miss Farmer has been kind enough to say that she asked me to say a word to-night because I knew and admired her father so well. I feel as though I were in a special sense in his presence, when I see here upon this platform these two delightful portraits, and I am especially glad that Mrs. Farmer's portrait has been put here too, for I know so well the services which she rendered him in all the work which he did, and among all the blessings which God has given to man there is none better than a faithful and true wife.

My first meeting with Mr. Farmer was many years ago, more perhaps than some of us who are getting along in years like to recall. There is one point in which our science at the present day seems to me to be quite deficient. There still is maintained an alliance between two of the sciences, between which I am sure a divorce ought to be decreed, and that is the alliance between the sciences of astronomy and physiology. I see no reason why physiologically we should get older in proportion to the number of times that the earth goes around the sun. It was in 1852 that I was an apprentice in a philosophical instrument store in Boston. I was sent to carry some apparatus which Mr. Farmer had there bought. I went to the fire alarm room in Court Square with this apparatus, and he at once received me very kindly and asked me if I knew what he was about. Well, I had a general idea, but nothing in detail, and he, busy as he was, took the opportunity of explaining to me the system of fire alarm telegraph which he was then arranging in Boston. We became friends. He called me Fred, and he called me Fred to the day of his death, and that is one of my most cherished memories. We all understand what this fire alarm system was, I will speak of it only in the personal connection. You will remember the system was designed to call out the districts on the alarm bell of the City, this information being sent to the central office by the simple operation of turning a crank on which there were projections, these projections indicating by a letter or number where the fire was. It was here that he began his work and it was here that he carried it through, and it was during the work that he did in that fire alarm office that he made use of the magneto-electric machine driven by water power for the purpose of sending in the call. After that he went to a room in Sudbury Street, and being in Boston about that time in connection with the Harvard Medical School, I used to spend a great deal of time in his laboratory. It was there that he developed the system of incandescent

lighting which has been frequently referred to. I remember this piece of apparatus very well. That tangent galvanometer which we see in the loan exhibit was one of the pieces of apparatus which he there employed. I went in there one day and he said, "look here." He took a little piece of platinum plate and cut a strip from it. He measured very carefully with a micrometer the length, breadth and thickness of the strip. Then by means of a formula he had worked out, he calculated the amount of current it was necessary to send through that strip in order to bring it to its melting point. He put the strip in circuit, turned the current on, and increased it until the point was reached on the tangent galvanometer giving that current, and the platinum became incandescent. He then said I will show you the correctness of my formula. He increased the current one degree and the platinum melted. It was in connection with that arrangement that he devised that movable rheostat by which if the current varies the rheostat took care of it and threw in resistance so that the platinum lamp should not receive too much current, a clear anticipation of the first devices made use of when the platinum lamps came into more extensive use. I have in my possession too, some of the bars of a thermo-battery which he made at that time, the metals being an alloy of antimony and zinc combined with a copper bar, etc. In regard to the dynamo machine which has been referred to, we all remember that the principle is that of self-excitation. You cannot put a piece of iron in any position whatever—in any but one position, without its being to some extent a magnet. The instrument which had been used for generating current hitherto, utilized permanent steel magnets. His idea was that any piece of iron is a magnet, and if you happened to put that piece of iron in the direction of a normal magnetic needle, it is more strongly a magnet. He said there is magnetism enough in every piece of iron for us to use it. Suppose we move a coil of wire near that bar, that coil of wire in consequence of that motion has a small current. Suppose I send that current around the bar; that will magnetize the bar and that will generate more current, and so on until the maximum is reached. There were three methods by which this was done, which Mr. Wild has told us, and as the Philosophical Society of Manchester clearly proved, antedate both Wheatstone and Siemens. So all the time this man was working quietly, piling up note-book after note-book and never publishing. I repeatedly asked him to publish. There is a line sometimes drawn between those who discover, and those who apply the discoveries of others to useful purposes; between the man of science, so-called, and the inventor. Now to a very large extent Mr. Farmer's utilizations were Mr. Farmer's discoveries. I remember very well in about 1860 there was a wonderful exhibition for that time of an electric light upon the evening of the 4th of July on the Boston State House. Prof. William B. Rogers had

charge of the exhibition and a very large battery was placed in dome, and wires were led up, and this light was shown at the same time with the fireworks on Boston Common. There was somebody else up there beside Prof. Rogers and his assistants and that man was Mr. Farmer. They wanted to make a display, they wanted to show an electric light larger than any that had been seen up to that time. Mr. Farmer had no such idea. He wanted to do what had not been done in the World before and what has not been done, possibly with one exception, since. He wanted to determine how much power, how much horse-power it required to produce that light, and out of the data which he obtained in those experiments in the dome of the Boston State House, on that night of the 4th of July, 1860, was the mechanical equivalent of light, and he gave us the fact that to support the light, not the heat, but the light of one candle required the expenditure of 13.1 foot pounds per minute. Some time after that, Prof. Thomson of Copenhagen, undertook from similar data to get a result, and the results were very closely accordant. Now this fact shows two things, and those are the only two things I want to say; first, that most of the matter that is in Mr. Farmer's note-books is matter which was new to science at that time. He was a great scientific discoverer. I urged him to publish, but that innate modesty of his prevented him from doing so, and consequently those things which were in his note-books are now shown as the practical outcome obtained by others, whereas we are striving to maintain that those note-books contain discoveries that should be credited to him. He was also a great inventor, and the inventions which he made you have evidence of in the fragmentary exhibit here, which is a very fine exhibit, but is far short of what it might have been had we been able to collect all Mr. Farmer's inventions and put them there. Now, may I not say, in view of this hasty résumé that I have given, that this atmosphere is an atmosphere to which it is well worth while to invite the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, and may I not say that the atmosphere in which Mr. Farmer lived and worked is an atmosphere where the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS' meeting cannot but receive an inspiration and an impulse. If I am right in this conclusion, then I have to return into my hypnotic state and to extend, in behalf of Miss Farmer and all the friends here who are conducting this splendid enterprise—to extend to my friend the President, Dr. Crocker, on behalf of the good people here a most hearty welcome to this hallowed spot and I am most happy to extend through him to the members of the organization the same welcome. [Applause.]

PROF. CROCKER:—Mr. Chairman, ladies and gentlemen—It is a matter of great satisfaction that the INSTITUTE is able to do some little honor to our late honorary member, Moses G. Farmer. He represents what I consider to be the ideal electrical engineer, be-

cause he combined the two branches of that subject, the theoretical and the practical. Dr. Barker has just pointed out that most of his work was done on the borderland between what we call pure physics and applied electricity. It would seem as if this combination of the two was most perfect and that either alone, without the other, is incomplete. The electrical engineer could not be a pure scientist, and while the latter might be a member of our body he would not represent the title in the most perfect manner. But in Prof. Farmer we have an ideal example of that well-balanced combination of pure science and useful application. Therefore we can do full homage to him, and the INSTITUTE will always cherish, I feel sure, not only his name but also it will be elevated by the example which he set to its members.

In taking up the subject of the evening, "The Precision of Electrical Engineering," it occurred to me as Dr. Barker was speaking that Prof. Farmer's mind had that very exactness which I am going to attempt to bring out. On several occasions his friends have spoken of the exact measurements which Prof. Farmer employed in his work, and Dr. Barker has just cited a case in which Prof. Farmer was able to predict the exact amount of current required to fuse a certain piece of platinum. It is that very precision, that very power of prediction, that I claim for our profession, and it seems therefore especially appropriate that I should now attempt to set forth this accuracy which characterized Prof. Farmer's work. The subject is obviously not a popular one. Nevertheless it is, I think, of general interest and the popular mind, as well as the scientific, is never tired of hearing of the wonderful achievements of electricity. It is a fascinating subject which never seems to lose its charm, and therefore I hope that some of the points will interest those present who are not electricians, although this address was of course prepared from the professional point of view. I may also state that I have not been able to do justice to the subject or to the occasion, owing to poor health during the last few months; but this theme is so powerful in itself that it simply requires to be started and it will carry itself along.

President Crocker then gave his address.

[See page 347, August-Sept. issue.]

The meeting then adjourned until the following day, when the paper presented by Dr. McFarlane, May 18th, was taken up for discussion.

[See page 163, May issue.]

TUESDAY, JULY 27, 1897.

DISCUSSION.

MR. CHAS. P. STEINMETZ:—I have been very much interested in this paper of Dr. Macfarlane's, since it offers a common method of calculation for the two different classes of phenomena which can take place, if a condenser discharges through a circuit of resistance and inductance. Such a condenser discharge can either be an oscillating current, that is, a current similar to an alternating current, but gradually decreasing in amplitude, or it may be a steady discharge, that is a gradual dying out without reversal. The former is the case if the resistance of the circuit is below, the latter if it is beyond a certain critical value.

The usual method of investigation of these phenomena leads to two different functions, an exponential function for the steady discharge which becomes imaginary if the resistance is below the critical value, and a trigonometric function for the oscillating discharge which becomes imaginary if the resistance is above the critical value.

Both these two forms of discharge are apparently entirely different from each other, nevertheless in reality they are of the same nature and gradually change into each other as you will best see by considering a mechanical analogon, as a pendulum in motion. A pendulum, when set in motion in air, will make a lesser or larger number of oscillations with gradually decreasing amplitude, until it comes to a standstill. The rapidity of decrease of oscillations depends upon the resistance of the medium in which it moves, thus for instance in a more resisting medium, as in water, the amplitude of oscillation of the pendulum will decrease much faster than in air and it will come to rest very shortly. In a still more viscous medium the oscillations will die out still more rapidly and only very few oscillations take place and ultimately only a half oscillation, that is, the pendulum will be brought to rest by the resistance of the medium without ever passing over the position of equilibrium. The latter case is analogous to the steady discharge.

It is very gratifying to see in the paper these two sides of the same phenomenon of condenser discharge, represented by the same symbolism.

There is, however, one sentence in the end of the paper in reference to a statement which I made once regarding the position of the complex imaginary quantity in algebra. I am sorry to say that I cannot agree with Dr. Macfarlane on this point, but have to maintain my former position.

All the eminent mathematicians of modern times as far as they have taken position at all, have considered the complex imaginary quantity as the most general algebraic quantity, and that there is no further extension of algebra possible outside of the complex imaginary quantity. It is obvious that these mathematicians cannot possibly have been mistaken.

In opposition hereto, Dr. Macfarlane takes the stand that there are other complex imaginary quantities possible outside of the complex imaginary one, and even offers here in the paper a complex imaginary or hyperbolic quantity outside of the complex quantity of pure mathematics. It is evident that there must be a mistake somewhere, and we can discover the mistake by looking on page 165 of Dr. Macfarlane's paper, where he introduced this hyperbolic complex imaginary quantity. He says there in the second line, "If we inquire into the geometrical meaning of the $\sqrt{-1}$ here appearing, we shall find that it means a quadrant of turning round the axis perpendicular to the plane of reference," then he introduces the hyperbolic quantity. This shows the source of the mistake. The quantity introduced here by Dr. Macfarlane is a vector symbolism, that is the symbolic representation of a plane vector, or in other words, of a geometrical relation, similar as for instance the system of quaternions is a space vector symbolism. A vector symbolism is not yet an algebraic quantity, for instance the quaternions are not algebraic quantities.

Undoubtedly the number of possible vector symbolisms is unlimited, the number of algebraic quantities is however limited.

We have thus to see what is the meaning of an algebraic quantity.

An algebraic quantity is a quantity with which we can operate by the common rules of algebra, that is, multiply, divide, solve equations, etc.

There is one fundamental principle underlying all algebraic operation, that is the principle that if a product is zero one of the factors must be zero.

Even the simplest calculations are based on this principle.

Take for instance the case that you have measured a resistance r and find,

$$5r = 20$$

everybody will say herefrom,

$$r = 4.$$

Explicitly the operations were,

$$\begin{aligned} 5r &= 20 \\ 5r - 20 &= 0 \\ 5(r - 4) &= 0 \\ r - 4 &= 0 \\ r &= 4 \end{aligned}$$

As you see in deriving this result $r = 4$ from $5r = 20$, a result which everybody would take for granted immediately, we have made use of the fundamental principle of algebra by cancelling with 5, that is assuming that if the product $5(r - 4) = 0$ and the one factor 5 is not zero, the other factor $r - 4$ must be zero. Thus if this principle does not hold, the calculation would

be erroneous, or in other words, every possibility of calculation would cease, and it is erroneous for quantities which are not algebraic, as for instance for the hyperbolic vector quantity introduced by Dr. Macfarlane.

Let us see now how the complex imaginary quantity stands with regard to this fundamental principle of algebraic quantities.

Let $a + j b$ be a complex imaginary quantity and $x + j y$ another complex imaginary quantity. Let their product be zero, that is,

$$(a + j b)(x + j y) = 0$$

If now $a + j b$ is a quantity differing from zero, the above mentioned fundamental principle of algebra requires that we can cancel the equation with $a + j b$ and thus get,

$$x + j y = 0$$

that is in other words, if $(a + j b)(x + j y) = 0$ and $a + j b$ differs from zero, $x + j y$ must be zero, if the complex quantity is an algebraic quantity.

From $(a + j b)(x + j y) = 0$ it follows:

$$(a x + j^2 b y) + j(a y + b x) = 0$$

or since $j^2 = -1$ by definition of the imaginary unit, it is

$$(a x - b y) + j(a y + b x) = 0$$

since, however, j is a different kind of unit, either of the two terms of the last equation must be zero, that is,

$$\begin{aligned} a x - b y &= 0 \\ a y + b x &= 0 \end{aligned}$$

Since a and b differ from zero, we can eliminate a and b from these two symmetrical equations and get the result,

$$x^2 + y^2 = 0$$

This condition can be fulfilled only by

$$\begin{aligned} x &= 0 \\ y &= 0 \end{aligned}$$

that means $x + j y = 0$.

Thence from the equation,

$$(a + j b)(x + j y) = 0$$

it follows,

$$x + j y = 0$$

that is the complex imaginary quantity is an algebraic quantity and can be treated as such.

Take, however, the hyperbolic complex quantity introduced by Dr. Macfarlane.

The hyperbolic imaginary unit of his is, $\sqrt{-1} \beta^H$ or $i \sqrt{-1}$.

For brevity, we may denote: $i \sqrt{-1} = k$. It is defined by,

$$k^2 = +1$$

assuming again the product of two such complex quantities equal zero:

$$(a + k b)(x + k y) = 0$$

it follows

$$(a x + k^2 b y) + k(a y + b x) = 0$$

or since $k^2 = +1$ and the two terms of the last equation are of different nature, it follows,

$$a x + b y = 0$$

$$a y + b x = 0$$

or, eliminating a and b , we get,

$$x^2 - y^2 = 0$$

that is,

$$x = \pm y$$

This means, if $x = \pm y$, the product $(a + k b)(x + k y)$ can be zero without either factor being zero.

Since x and y differ from zero it follows analogously, as condition of zero value of the above given product, that,

$$a = \mp b$$

that is, if in two hyperbolic complex imaginary quantities the coefficient of the real term equals the coefficient of the imaginary term, but the signs of the imaginary components are opposite, the product of these two hyperbolic complex quantities is zero without either factor being zero.

$$(a \pm k a)(x \mp k x) = 0$$

Furthermore it follows that the hyperbolic complex imaginary quantity of this paper is not an algebraic quantity, and that equations in such quantities cannot be handled by the laws of algebra, for instance, cannot be cancelled by a constant factor without being liable to see an equality changed into an inequality. That means the hyperbolic imaginary quantity, while a vector symbolism, is not an algebraic quantity, and my statement that the complex imaginary quantity is the only and most complete algebraic quantity still holds.

DR. A. E. KENNELLY:—I am sorry that Dr. Macfarlane is not with us, because we might expect an animated discussion on the points Mr. Steinmetz has raised. There are several matters of great interest in this paper. I will only venture to call attention to two of them.

We all know that if a plane cuts a right cone at right angles to its axis, the curve of intersection is a circle. If the cutting plane is tilted, the circle becomes an ellipse; until, when the plane is parallel to one side of the cone the curve becomes a parabola; and, finally when the plane is tilted beyond this position the curve is a hyperbola.

The results pointed out by the author may be expressed as follows: When a perfect condenser discharges through a perfectly conducting circuit, *i. e.*, a resistanceless circuit, containing induc-

tance, the discharge is oscillatory without damping, and may be represented by the motion of a point in an ellipse with equiangular velocity around the centre, the angle being measured elliptically; or, by the motion of a point in a circle with equiangular velocity, as a particular case of the ellipse.

When the discharging circuit is not perfectly conducting, but offers a resistance, such that its magnetic time-constant is greater than one-fourth of the static time-constant, the discharge, no longer undamped, may be represented by equiangular velocity in an ellipse—with the circle as a particular case—accompanied by a logarithmic shrinking of the radius vector. The trace of the point's motion is therefore a logarithmic spiral, or an orthogonal projection of the same on an inclined plane, *i. e.*, a logarithmic elliptical spiral.

When the discharging circuit has so much resistance that the magnetic time-constant is greater than one-fourth of the static time-constant, the cutting plane of the cone must be tilted from the elliptic curve to the hyperbolic curve. The discharge of the condenser may now be represented by equiangular velocity in the hyperbola, a rectangular hyperbola as a particular case, accompanied by logarithmic shrinking of the radius vector. The trace of the point's motion is therefore a logarithmic hyperbolic spiral.

When the discharging circuit has critical resistance, so that the magnetic time constant l/r is one-fourth of the static time constant or $cr/4$, the cutting plane must occupy the intermediate position, and the curve becomes a parabola. The discharge of the condenser may now be represented by equiangular velocity in the parabola, accompanied by logarithmic shrinking of the radius vector. The trace of the point's motion is therefore a logarithmic parabolic spiral.

I do not mean that the above statements are all in the paper before us, although some of them are there. I mean that these are conclusions to which the very interesting treatment in the paper appears to lead.

The second point is of less interest to us as electricians, it is at present almost wholly of mathematical interest. The square root of minus unity was originally called imaginary, because it was regarded as a logical reduction to an impossible or unusual result. Later it became readily interpreted geometrically as the operator which rotates a line counter-clockwise in a plane through a right angle. No other interpretation has hitherto been given to this symbol so far as I know. Dr. Macfarlane now points out that it is also capable of a new meaning in which it is not a versor but a scalar.

MR. STEINMETZ:—I may add that whatever criticisms I have made does not apply to the particular application of hyperbolic imaginary quantities made by Dr. Macfarlane, since in his paper these complex quantities are never used as products, as for instance, common complex imaginary quantities are used when

deriving $E. M. F.$ as product of current, impedance, etc. It is only in this latter case where it becomes essential that the complex quantity is an algebraic quantity.

DR. F. A. C. PERRINE:—There is just one point—in speaking of complex quantities, which are really not the subject of the paper, but nevertheless have been brought up in this discussion—in respect to which I think there is an error that has been commonly committed and has interfered with the general understanding of the complex quantity, and that is the error of considering the introduction of the complex quantity solely, as Mr. Kennelly says, for the geometrical interpretation of a particular mathematical symbol, and that our interpretation of the complex quantity is that if we plot along one axis the value j , and on the other axis the value of the real quantity—we will call it a —than any vector may be represented by a sum which we will call $a + bj$. Now additions or subtractions may be performed entirely geometrically. Let us attempt to obtain a multiplication. Take another vector which we will call $c + dj$, and commonly we refer this to a unit vector in order to obtain multiplication, take this point as the unit vector, then the multiplication of these two quantities is performed by drawing on the first vector a triangle entirely similar to the triangle drawn by joining the end of the unit vector and the second vector. This will give us a third vector which, in the geometrical interpretation, has been sometimes called the product of these two vectors. Now as an actual fact, that is not the product which we can use in our analytical working, for the real product of these two vectors is the product of their scalar values times the cosine of the angle between them, which is an entirely different product from this geometrical form of product. This is an error I have never seen pointed out, though I have noticed more than one writer, shortly after the World's Fair, who began to explain the system, and after reaching geometrical multiplication was compelled to stop, because the saying that this vector representation of the complex quantity is a geometrical method of interpreting the square root of minus one, fails as soon as we accept the geometrical method of multiplication which is the only method of multiplication of the complex vectors which has been at all described in the chapters in the algebras on the complex quantities, and it leads to entirely wrong results, and it shows at once that the true method of dealing with the complex quantities is an algebraic method and not at all a geometric method.

MR. STEINMETZ:—I agree with the last speaker that in multiplying complex imaginary quantities we have to deal algebraically—since in multiplying two complex imaginary quantities geometrically, the product of the two quantities may have no meaning.

In the geometrical multiplication the product of two complex quantities as vectors is a vector again. Wherever the product of

the two quantities represented by the complex numbers is not a vector quantity, it is obviously not feasible to represent it in the diagram by a vector, and thus the geometrical multiplication becomes meaningless. This is for instance the case when multiplying current and *E. M. F.* to derive the power, since power is of double frequency and thus cannot be represented in the polar vector diagram of single frequency.

DR. PERRINE:—That is perfectly true; but at the same time it is not as much a foregone conclusion as you might think; because I was taught to multiply these complex quantities in that way by a man who had learned it from no less a man than Weyerstrass in Germany, and this was given to me as the sum of knowledge in regard to complex quantities, and I think that a great many learners have been confused by exactly that error in the geometrical interpretation of complex quantities.

MR. STEINMETZ:—The geometrical multiplication of complex quantities is obviously just as correct as the algebraical multiplication, wherever the product can be represented geometrically, that is wherever the product is a vector, as for instance when multiplying current with impedance or *E. M. F.* with admittance. Only where the product of the two vectors is not a vector, as when multiplying current and *E. M. F.*, the geometrical multiplication which gives a vector as product, becomes meaningless.

DR. LOUIS BELL:—It seems to me that this discussion gives a very beautiful example of how not to use certain mathematical symbols. I think we owe to Mr. Steinmetz a great deal for his clever manipulation of the imaginary quantity in the solution of physical problems, but I do not know that anything has been brought out more clearly in this discussion, than the necessity of tying yourself up to your physical conceptions. Whenever you let yourself loose and deal with imaginary quantities which have not a close and precise physical meaning, you are apt to get into trouble. Just so long as you tie yourself up, keep out your sheet anchor, look sharply as to the character of the quantities with which you are dealing, you will derive very valuable results. As soon as you cease remembering that these imaginary quantities are means to an end, that they form a vastly convenient and very precise method of dealing with certain physical things—as soon as you forget that, and start with a free hand to swing your complex quantities for the purpose of seeing what transcendental results you can get, then you are almost ready to get into difficulty. Mr. Steinmetz has given us some beautiful examples of the way in which it should be used, and Dr. Perrine has certainly given one or two examples of the way in which it should not be used. That fact runs all through our physical mathematics; that as soon as you get out of sight of physical interpretations and start out with a free sheet your cruise may bring you to some place which you desire to reach; it may land you in a place from which nothing but an all-wise providence can extricate you.

[COMMUNICATED AFTER ADJOURNMENT BY DR. A. MACFARLANE.]

[See p. 163 *et seq.* for paper and discussion.]

I regret that I was unable to be present at the General Meeting of the INSTITUTE, but I am glad to have an opportunity of stating by correspondence the reply which I would have made to the points which were advanced in the discussion. First of all, I wish to thank Mr. Steinmetz and Dr. Kennelly for the favorable opinion which they express of the investigation proper; divergence of opinion exists only with respect to the bearing which the investigation has on certain principles of mathematical analysis.

The position in the paper which Mr. Steinmetz attacks may be thus stated. If the investigation in the paper is correct, then the current doctrine about the field and limits of algebra is incorrect. In order to state impartially the current doctrine, I took the very concise statement of it given by Mr. Steinmetz in his work on "Alternating Current Phenomena." It may be well to quote the paragraph in full: "Thus starting from the absolute integral numbers of experience, by the two conditions,

1st. Possibility of carrying out the algebraic operations, and their reverse operations under all conditions,

2nd. Permanence of the laws of calculation, the expansion of the system of numbers has become necessary into,

Positive and negative numbers,

Integral numbers and fractions,

Rational and irrational numbers,

Real and imaginary numbers, and complex imaginary numbers.

Therewith closes the field of algebra, and all the algebraic operations, and their reverse operations can be carried out irrespective of the values of terms entering the operation. Thus within the range of algebra no further extension of the system of numbers is necessary or possible, and the most general number is, $a + j b$, where a and b can be integers or fractions, positive or negative, rational or irrational" (p. 404).

Mr. Steinmetz and I agree in holding that the above is the view of the eminent mathematicians of modern times, but he holds that "it is obvious that they cannot possibly have been mistaken," while I hold that the investigation in the paper demonstrates that they have been mistaken.

Let us consider now the reasoning by means of which Mr. Steinmetz in his reply supports the current theory, and endeavors to show that the hyperbolic complex quantity is not an algebraic quantity. To simplify the question in dispute, we may accept the definition of an algebraic quantity which he gives, and admit the whole investigation down to $x^2 - y^2 = 0$. He assumes, and on this assumption hinges the whole matter, that this equation can be satisfied by finite values of x and y provided they are equal. But as x and y are the terms of a hyperbolic complex quantity, it is impossible for them to be equal, if they

are finite quantities; in other words $\sinh \varphi$ is necessarily less than $\cosh \varphi$. The hyperbolic complex quantities considered in the paper (p. 164) are $-a + \sqrt{a^2 - b}$ and $-a - \sqrt{a^2 - b}$; the quantity under the radical sign is necessarily less than the other. The expression $x^2 - y^2$ means the square of the modulus of the hyperbolic complex quantity, and as x and y cannot be equal for any finite value, the equation $x^2 - y^2 = 0$ can only be satisfied by both x and y being 0. In the case of the hyperbolic complex quantity, the two terms are at right angles to one another, and the vanishing of the modulus involves the vanishing of both terms for the same reason as in the case of the circular complex quantity. For the hyperbolic complex quantity the modulus $r = \sqrt{x^2 - y^2}$ and the hyperbolic angle φ is such that $\tanh \varphi = y/x$. Let using Kennelly's angle notation, one hyperbolic complex quantity be denoted by r / φ and another be denoted by r^1 / φ^1 , then the product is $r r^1 / \varphi \varphi^1$ precisely after the manner of the ordinary (circular) complex quantity. If the product vanishes and r does not, then r^1 must vanish, and it can vanish only by x^1 and y^1 each vanishing. Hence the hyperbolic complex quantity is an algebraic quantity according to Steinmetz's own definition of such a quantity.

The investigation in the paper shows that the hyperbolic complex quantity is precisely analogue to the ordinary (circular) complex quantity; and for any theorem in circular trigonometrical analysis, there is a corresponding one in hyperbolic trigonometrical analysis. If the former is algebra, how is it possible that the latter cannot be algebra?

If my paper demonstrates anything, it demonstrates that the accepted theory of the quadratic equation requires to be improved in the way of extension; and one may reason that since the eminent mathematicians of modern times have not seen the complete theory of the quadratic equation, it is quite possible that they may have been mistaken about the field and bounds of algebra. The view of the quadratic equation which was held by the mathematicians of less modern times, was that if the quantity under the radical sign was positive, the solution was possible, but if it was negative, the solution was impossible. This is the true and complete theory, when the unknown quantity x is a linear quantity. However, when x is a quantity in a plane, the roots are possible, whether the quantity under the radical sign is negative or positive; in the former case, we have the circular complex quantity, and in the latter the hyperbolic complex quantity. The fact that the real roots are also capable of representation in the plane, is the part of the theory which has been missed by analysts, and is advanced for the first time in my paper.

For instance consider the quadratic equation

$$x^2 - 5x + 6 = 0.$$

If x is a linear quantity, it is either 3 or 2; but if it is a planar

quantity it is either $\frac{1}{2} + \frac{1}{2}$ or $\frac{1}{2} - \frac{1}{2}$, where the $\frac{1}{2}$ is the abscissa and $\frac{1}{2}$ is the ordinate of an equilateral hyperbola. I may mention that since the paper was read before the INSTITUTE, I have applied this theory of the quadratic equation to the solution of the cubic equation, and have contributed the results to the Toronto meeting of the British Association.

Dr. Kennelly states very clearly some of the results to which the investigation in the paper leads. He omits by inadvertence to mention that it is the projection on the vertical axis of the motion described, which represents the electrical phenomenon. As regards the scalar $\sqrt{-1}$, we shall see that there must be such, if we investigate the proper analytical expression for a hyperbolic complex quantity. Suppose that the two components are x and y ; as y is at right angles to x , it must be affected by the vector $\sqrt{-1}$ which may be denoted as usual by i ; but as the modulus is $\sqrt{x^2 - y^2}$ instead of $\sqrt{x^2 + y^2}$ it may be reduced to the latter form by writing it $\sqrt{x^2 + (\sqrt{-1} y)^2}$. Hence the proper expression for the hyperbolic complex quantity is $x + i \sqrt{-1} y$. The $\sqrt{-1}$ here introduced is evidently scalar in nature. In my paper on the "Principles of Elliptic and Hyperbolic Analysis," I have advanced other reasons for believing in it. (p. 20).

The mistake to which Dr. Perrine refers is very apt to be made by one who does not perceive that a vector and a complex quantity are not the same thing. The former is a directed line, the latter is an angle associated with a multiplier. It is an entire mistake to call $a + bj$ a vector; its geometrical meaning is, an angle associated with a multiplier; and the product of two such quantities is another angle, which is the sum of the given angles, and a multiplier which is the product of the given multipliers. To say as do Dr. Perrine and Mr. Steinmetz that this product has no geometrical meaning is an entire mistake. When the multipliers are reduced to unity, the product expresses the fundamental theorem of circular trigonometry. Similarly the product of two hyperbolic complex quantities, when the multipliers are unity, expresses the fundamental theorem of hyperbolic trigonometrical analysis. Of course the product of two vectors is an entirely different matter.

THE PRESIDENT:—If there is no further discussion of this paper we will pass to the next paper on the list, "The Effect of Heat Upon Insulating Materials," by Putnam A. Bates and Walter C. Barnes of New York. Mr. Bates will please present the paper.

[Mr. Bates read the paper which is printed at page 215 May Issue of TRANSACTIONS.]

DISCUSSION.

THE PRESIDENT:—This paper, as stated in the introduction, is the result of work taken up in order to reconcile the extreme differences that were pointed out by Mr. C. F. Scott when the paper by Messrs. Sever, Monell and Perry was read at the last General Meeting, May 20th, 1896. It seemed to me that leaving on the records of the INSTITUTE a practical and important matter of this kind with such great differences of opinion was extremely undesirable, and therefore the attempt was made to ascertain the true facts. Mr. Scott made the statement that experiments with which he was familiar, obtained the minimum resistance exactly where the other investigators had obtained the maximum value. That discrepancy is entirely explained in the way that I suggested in the discussion of the paper at the time, by the curve shown in Fig. 2, when you will note that the maximum in curve No. 4 is about 85°C. , and the minimum in curve No. 3 is at 80°C. That difference is entirely due to the fact that in one case, curve No. 3, the material was wound with copper wire, the interstices between the turns allowing moisture to escape somewhat freely. In the other case where the maximum as well as minimum points were shifted to the right, the insulating material was wound with sheet iron, and a much longer cylinder of insulating material was tested; consequently the opportunity for escape of moisture was much less, and the result was obtained just as expected. But the fact that the maximum and minimum points can be shifted to the right or to the left in this way is absolutely proved and is, I think, a matter of some practical importance. Furthermore, it is also shown in the curve given in Fig. 3, that the behavior of this insulating material between 20° and 80°C. is simply dependent upon the moisture that it contained, and that after driving off that moisture and allowing it to return, the material regains exactly the same condition as before. Therefore we can control entirely between those limits the position of the curve by the amount of moisture that we allow to enter the material after having driven it out. Those two points, I think, are of interest and importance. The curve shown in Fig. 4 represents the effect produced by much higher heating up to 200°C. , and while very high resistances are obtained, running into the hundreds of thousands of megohms per square inch, it should be remembered, as pointed out in the paper, that the mechanical strength of the insulating material is very much affected, and that while we get what might be called laboratory results of high resistance, the material is not in a condition which would make it at all suitable for use in practical electrical machinery; but up to 80°C. , which is the ordinary limit, apparently, no permanent effect is produced. It is simply a question of how much moisture is allowed to enter and how much to escape. The exact limits at which material becomes permanently injured is not an electrical question; it is a mechanical one; because the

insulation resistance is very high even after the material is badly affected; hence, it is rather difficult to set a limit. Probably some test as to the strength of material, its tensile strength for example, would be the best way to determine that point,—or its resistance to mechanical puncturing or possibly electric puncturing. That however has not been done. But the question of the effect of temperature upon insulating material is one of vital importance in electrical engineering, because our most serious difficulties arises from the breaking down of insulation, very often due to excessive temperature.

The paper is open for discussion.

DR. PERRINE:—Both at the time the earlier paper was read and at the time I first saw this paper it was a matter of great surprise to me that there should not have been more knowledge of these facts disclosed by the electrical engineers, particularly as I knew that there were so many men in the body who were familiar with work in paper cables and similar experiments where the investigation of moisture in insulation was made necessary. What this paper proves, and what should be more or less a foregone conclusion, is that if you have moisture in the insulation the temperature co-efficient is a co-efficient largely of the moisture itself, and furthermore, that the amount of moisture determines the specific insulating resistance of the material. Again we know that these materials, such as fibre and paper retain in comparatively dry air, a large amount of moisture. In drying paper insulated cables, taking a 100 pair cable wrapped to 128/1000ths. of an inch of paper, and using No. 18 wire; more than four per cent. of the weight of the combined paper and copper is in the moisture which may be driven off by proper drying, and in the paper itself, from 15 to 20 per cent. is moisture, in the driest air you can find. Last year I had some experiments tried in summer in California, where the air is drier than we almost ever see it here, and wood that was thoroughly dry would in two days absorb from 13 to 15 per cent. of its weight in moisture. This being true it is not by any means surprising that the temperature co-efficient of any material would largely depend upon the moisture contained in it. Compressed paper and fibre hold less moisture than wood or the ordinary manila paper used in wrapping cables. But at the same time all of us who have been through the harassing experiments in making commutators with fibre which was the practice ten years ago, know that the best of the compressed paper and the best of the fibre that we can obtain, contains enough moisture for it to change in its dimensions after being dried out. Not less than five per cent. of the weight of the best compressed paper made to-day is water in dry air. It does not need exposure to moist air. But air as dry as this or drier will enable the best quality of compressed paper to hold at least five per cent. of its weight in moisture. Now this being so, as I say, it is not at all surprising

that we should find the temperature co-efficient of the insulating material, under such conditions, is a matter of the amount of moisture in it, and it is surprising to me that experiments should have been made within the last five years on insulating materials which intended to give the temperature co-efficients of the insulating materials themselves, and no effort made to remove the moisture before the test were commenced. The matter of removal of moisture is also a difficult matter. The curves on page 221 show that. On repeated heating the resistance rises and rises, and every time you heat it, even keeping the specimen in what you call dry air of the receptacle in the laboratory—not simply exposing it to the dry air of the laboratory, but keeping it as nearly dry as possible, and by repeated heating you will drive off a little more moisture each time you bring the temperature up, and in consequence the insulation resistance will rise, finally approaching a maximum. Again, we know from our experiments in paper cable working that it is not necessary to destroy the mechanical properties of the insulation in drying it out; for it is not necessary to dry paper or fibre or anything else above 100° C., or even as high as 100° C., if we dry by means of perfectly dry air in a desiccator; and that is the only means by which the moisture can be eliminated with any degree of completeness at all. The attempt to drive off moisture by heating is a question of the humidity of the atmosphere with which your specimen is surrounded. In the ordinary baking ovens of the electric factories—at any rate as they were made ten years ago, when they were simply baking ovens—we found, that in the cold dry weather of the winter-time we could dry out armatures in 24 hours, more completely than we could dry them out in a week in the summer time, when the atmosphere was moist, and although the same difference of temperature between the outside and inside air was obtained. It is simply a fact that the air has a certain capacity for absorbing moisture, and so has the insulating material a capacity for holding it, and the question of desiccation depends upon the relative amounts of moisture held by the insulating material and by the air. If your air at a certain temperature is saturated, the air will not dry out the insulating material. If you raise the temperature of the air and the air has been previously saturated, you then increase the capacity for moisture of the air slightly, but you cannot thoroughly dry out an insulating material, unless you pass over it air which has previously been thoroughly dry, and when you do this there is no necessity to raise the temperature of the insulating material so as to destroy its mechanical quality. Indeed in the cold, we may thoroughly dry out an insulating material by the use of a desiccating material if we give it time enough. The application of heat simply hastens the time of removal of the moisture, and in consequence the application of a certain amount of heat is advisable, but it is not by any means necessary. The moisture

within the insulating material and in the air is in a constant state of interchange, just as there is an interchange with a glass of water standing in the air. As we have a certain vapor density, that vapor density is not changed by the fact that moisture is absorbed in the interstices of the insulating material, and if the moisture goes out and is absorbed by some desiccating material, then we have a vapor vacuum for the remainder of the moisture, and more will be given off until finally our insulating material becomes dry, and that without producing any mechanical injury whatever. There is nothing new, as I say, about these facts I am giving you. This has been the experience with all paper cables made during the past ten years. Any cable manufacturer knows that it is perfectly easy to talk from one wire to another in a paper cable as soon as it has been laid together in the factory before it has been dried out. With a telephone it is sometimes difficult on a thousand feet of cable to distinguish between wires. This is simply on account of the presence of moisture in the insulating material, and that difficulty is entirely removed, and we begin to get insulation only after the moisture has been removed.

So far as the temperature co-efficient is concerned, moisture being present as shown here, it is not a temperature co-efficient of the insulating material at all. This paper simply confirms our knowledge of the fact that compressed paper and fibre absorb moisture readily, and are unreliable as insulating material—a knowledge which has altered the construction of most of our electrical machinery, for where great reliability was necessary, compressed paper and fibre have been entirely removed, and not within at least five years, perhaps, have these materials been used where reliability was considered necessary, and that simply because the presence of moisture in the material has been understood as the reason that compressed paper was not a reliable insulating material.

THE PRESIDENT:—Of course it has been known for many years that the effect of moisture on insulating materials was deleterious, and furthermore, that all such materials as paper, cotton etc., contained a great deal of moisture, and that they would absorb it from the air and retain it, and even though the air was what we commonly call dry, they still contained a large percentage of moisture. That general fact is common knowledge; but it was not known generally that the effect between 20° and 80° C. was due to nothing but moisture; that there was practically no temperature co-efficient. Furthermore, a year ago, at the meeting of the INSTITUTE, no one was able to explain the extraordinary discrepancy between the results given in the paper then presented, and the results cited by Mr. Scott. The experience with paper cables which Dr. Perrine has had, makes him especially familiar with this subject, but all of us have not had that experience. Furthermore, I would suggest that it would have been desirable if Dr. Perrine at some time had presented a paper to the INSTITUTE on this subject.

DR. PERRINE:—I am very sorry that I did not. The only reason I had for not doing so was because I thought it was common physical knowledge. Open any text-book and you will find it stated there that wood and paper will hold in dry air from 15 to 25 per cent. of moisture, and it seems to me when one-sixth at least of the insulating material is water, we would naturally look to water as giving us our temperature co-efficient, and it did not occur to me at the time I was working at the thing that there was anything new about it at all, because you will not find a book on fuels that does not give you that information.

THE PRESIDENT:—That is a general qualitative fact, Dr Perrine. I am talking about the specific curves that are obtained for insulating materials, and as I say, a meeting of the INSTITUTE at which many prominent members were present, including those particularly engaged in this line of work was unable to solve the problem. That is a matter of record.

DR. PERRINE:—I will tell you where I think you will find this thing—not exactly in curves, but you will find statements of the fact. In 1862 there was a conference called in relation to the possible laying of an Atlantic cable, where insulating materials were discussed, and this question of moisture in its value of changing the insulating material and the temperature co-efficient in insulating materials was pointed out in connection with some of the points then presented before the telegraph conference. It is printed in one of the English blue books, and I think you will find most of this material there.

THE PRESIDENT:—The general fact is one thing, as I say, and the specific curves another. Dr. Perrine is continually stating that it is a matter of common knowledge. Ever since dynamos have been used they have been provided with water-proof covers for the reason that the actual dripping of water upon them is most objectionable. But that is not exact physical knowledge, and I simply refer to the TRANSACTIONS of last year for proof of the fact that a largely attended meeting of the INSTITUTE could not explain the discrepancy there brought out.

PROF. THOMSON:—I think there is no question that as a qualitative fact it has been known that the presence of moisture is to be avoided in insulating material, and also the fact that cellulose if sufficiently heated becomes a dielectric, and will remain charged—the old experiments with rubbed paper prove that; still I think that the paper has brought out and put in a precise shape just how variations of insulation have occurred. Whether these matters have been fully investigated or not, and published in other papers I am not aware. I have not come across any curves of this kind. There are, however, other facts in connection with cellulose as an insulator, which I think have made it less desirable than other materials. It is a substance which is not by any means stable at high temperatures; even a temperature below 100° C., runs it down or carbonizes it slightly, so that finally

it becomes an entirely different chemical substance. Some experiments were made several years ago, I think as much as ten years ago for that matter, in the Thomson-Houston factory. We subjected cotton covered wire to varying temperatures for long periods of time, and found that even at 100°C , there was a gradual deterioration and that at any point much above 100 it was pretty rapid. The cotton turned brown and showed every evidence of a permanent loss of water and an increase of carbon relatively to the hydrogen and oxygen. We all know as a fact that browning and deterioration actually occurs at ordinary temperatures through very long periods of time. We see the old books in the libraries turn brown from the same cause. The substance cellulose, in other words is not stable, and a high temperature simply brings out the instability or makes the time required to change the substance less.

PROF. WILLIAM ESTY:—I enjoyed hearing the discussion of the paper and the paper itself, and would just like so say that at the University of Illinois, two of our seniors last year made very similar experiments on different materials, giving results that agree substantially with those published in these TRANSACTIONS. There was one point of difference in our experiments, and that was in the method used to heat the materials. Here it was an electrical method, using a number of resistance coils, whereas we used a cast-iron cylindrical vessel in which the material, in the form of a circular disk, was placed between two iron plates. Then a sort of cylinder-head top was screwed on to this flat vessel and the whole immersed in boiling oil or heated oil I should say. The temperature at starting, of course, was about that of the room, the cylinder oil being heated slightly by means of two or three gas jets—Bunsen burners—and the temperature could be increased quite regularly, although it was a rather slow process, till 100 or more degrees centigrade was reached. It took a good deal of time of course to get the temperature where it was desired, but the method, as I say, was quite satisfactory.

There is one other point I wanted to find out about in this paper, and that was the source of electromotive force. It was stated I think at 500 volts,—but not the source of E. M. F.—on page 216. I would like to ask Mr. Bates what was used.

MR. BATES:—A motor dynamo was used to transform the potential from 110 to 500.

PROF. ESTY:—We found on trying a 500-volt dynamo that the galvanometer gave quite unsteady readings. The circuit acted as though it had electrostatic capacity, as doubtless it had. We had a sort of condenser effect, with those two plates, upper and lower, with the insulating material between them as a dielectric, and we found that the unsteadiness in running the dynamo made it absolutely impossible to get any reliable results. The galvanometer needle if once started to the right would continue to the right, but by getting it once started to the left it would go to the left;

so that what we finally adopted was a storage battery, one that we made—of small cells and a good many of them. We got up to 600 volts, which was quite satisfactory, and all disturbances of the galvanometer were eliminated. Another thing we found that required a great deal of attention, and of course that goes without saying, that the insulation of all the apparatus—glass insulation—could not be depended upon in the ordinary way. But all the circuits having been made air lines and everything arranged with the utmost precaution to avoid leakage anywhere, the results we obtained on a number of substances, linen and fibre and the ordinary insulating materials, were substantially the same results as shown in this paper; that is the effect of moisture, and again the effect of pressure between the two plates, between which the material was placed, made a great deal of difference, of course. The way we tried to reduce the results to a uniform basis was to take a five pound lead weight, place it on the upper plate and use the same weight throughout all the experiments. That eliminated any possible uncertainty as to the pressure which would be introduced by wrapping the material on to a cylinder by means of a wire, unless that wrapping produced just the same pressure in every case, otherwise it would hardly be correct, it seems to me, to draw conclusions from these different conditions; but this uniform weight enabled us to say practically that all material was subject to exactly the same pressure. It is needless to say that by adding to the pressure we found the insulation resistance would drop very quickly, and removing the weight, of course, brought up the insulation resistance.

There is just one other point that I wanted to speak of, and that was what seemed to me desirable in the reduction of these results, viz. insulation resistance per square inch. I think this is the first time I have seen that reduction to unit of area, but should think that the addition of the thickness to that unit might be desirable. For instance, I would plot megohms per square inch per mil thickness, and then we could compare all our results and perhaps get conclusions more readily than where these units differ. I think in the former paper that is referred to here, the one by Messrs. Sever, Perry and Monell, that no reduction whatever to a unit was given—but simply megohms, and here megohms per square inch. In the results that we got last year at the University of Illinois we reduced to megohms per square inch per mil of thickness.

MR. STEINMETZ:—With reference to the instability of the galvanometer dynamometer observed by the last speaker, I observed the same phenomenon some years ago and found the source of it in the mutual induction between the two coils of the instrument which, with a leading current load as a condenser, produced an unstable equilibrium. Now coming to the paper—this paper is very interesting and valuable in-so-far as it extends the amount of information on insulating material available to the electrical fra-

ternity by publication. There are three distinctly different phenomena taking place in the insulating material. The one is the changing of resistance of the insulating material proper, with temperature; the second is the effect of the moisture retained by the insulating material, and the third is the chemical disintegration taking place. I may say that the separation of these three phenomena would be very desirable. It would be much more easily accomplished by using instead of the "resistances" the "conductivities" of the insulating materials. Then, I think, we could with some observations separate the three distinct effects, and get the electric conductivity of the insulating material proper, and such curves, of exponential or logarithmic nature, have been, I believe, observed on materials which are not hygroscopic, as hard rubber, guttapercha, etc. A further conductivity, representing a resistance increasing with the temperature, not linear but rather erratic, superimposed upon the conductivity of the insulating material proper, is the conductivity due to moisture included by the material, and this as before said is entirely erratic, depending absolutely on the atmospheric conditions and the chance of the moisture to be absorbed or to escape. Furthermore there is a chemical disintegration taking place which changes the material and which depends again on temperature and on time.

I will draw attention to one feature which may very easily cause misunderstanding, namely, the effect of chemical disintegration in lowering the insulation resistance, and the effect of moisture in lowering it. There is an enormous difference in their practical bearing. Moisture, while reducing the insulation resistance, is, within a reasonable range, perfectly harmless. It is useless to exclude it, because as soon as the electric machines come into operation they will absorb moisture again just the same.

Thus the only object of expelling moisture by baking is to get a sufficiently high number of megohms insulation resistance, to fulfil some antiquated set of specifications.

I am glad to state that more and more consulting electrical engineers come back from this question of the megohms insulation resistance as a guarantee of the safety of insulation of the machine, and instead of it specify the voltage at which the insulation should not be punctured. If the insulation is fibre and is very bad, it will be punctured at low voltage and still, after baking showed an enormous insulation resistance, while a very good insulation, after long exposure to damp air—as in a turbine station—may be of relatively low insulating resistance and nevertheless far superior to the infinite number of megohms which break down by disintegration or puncture at low voltage. Fortunately now you very seldom find an electrical engineer sufficiently behind the times to request a large number of megohms. If this is the case he gets it by baking the apparatus—otherwise baking goes more and more out of use, and its place is taken by the high voltage or puncturing test.

Obviously moisture, while fairly harmless when in small amount, must not be excessive—that is the armature must not be dripping with water, which all the same happens occasionally, when machinery shuts down evenings, and the dew accumulates on it over night. Even then it usually does no harm. So you see you must distinguish between the low insulating resistance due to moisture, and that due to imperfect material or chemical disintegration.

PROF. THOMSON:—There is just one point I would like to call attention to—I meant to speak of it when I was on my feet before—and it is in regard to the curves on page 221, Figure 3. It would appear that when the moisture was dried out of the material and the cooling took place, that there was no temperature co-efficient properly so-called. No. 9 and No. 11 are practically straight lines and that would seem to negative any idea of a variation of resistance, at least within those limits due to temperature of the dry material.

DR. KENNELLY:—The discussion has been interesting in showing the different standpoints which may be assumed by different readers who have not tacitly agreed upon the exact question at issue. When the original paper, referred to at the outset, was read, I think I can remember that the subject was not treated as a discussion of a true physical constant i. e. a temperature co-efficient of variation in resistivity of heated insulating materials, because it was generally understood, although perhaps not stated, that the variations in resistance observed were much more erratic than could be expected from a true temperature-co-efficient. As Mr. Steinmetz says, you cannot expect to come within measuring distance of a temperature co-efficient until you have driven off practically all the moisture from the test piece. Prof. Thomson has pointed out that, apparently, when this was done, the curves Nos. 9 and 11 of this paper indicate an absence of temperature variation between the limits of 20° and 80° . Dr. Perrine, however, seems to have considered that we discussed the existence of a real temperature co-efficient when the original paper was read. It is, of course, generally known that paper and fibre absorb much moisture at ordinary temperatures, and in the presence of that moisture all real temperature variations are necessarily obscured. The question discussed was why the particular curves accompanying that paper showed humps or maxima at positions very different to those observed by Mr. Scott. No satisfactory solution of this question seemed to be offered. I think we are indebted to the writers of this paper for an explanation of that phenomenon. They seem to show that everything depends upon the facility and speed with which the moisture is driven off in the test.

We cannot suppose that a material such as cellulose can have a real resistivity unless a number of conditions are stated, such as absence of moisture, pressure, degree of purity, etc. For this reason, perhaps, we should be grateful to the authors of this paper

for having referred their results to absolute insulations only, and for not having computed them as resistivities referred to the cubic centimetre. For had they stated their results in the form of resistivity, we might have been inveigled into the belief that such was a real physical constant of the material; whereas it was probably a function of a number of local conditions any variation in which might have led to a different numerical result.

The following paper on "The Effect of Armature Induction upon the Electromotive Force Curves of an Alternator" was then read by Prof. W. E. Goldsborough (see p. 227, June and July issues).

DISCUSSION.

THE CHAIRMAN [Vice President Steinmetz]:—This paper has been very interesting to me in-so-far as it gives one of the various effects taking place in the armature of an alternator separate from the others. Here we have an alternator having a very strong self-induction in the armature, with practically no armature reaction, that is no magnetizing effect of the armature on the magnetic field. You see here separate from all the other effects that of the self-induction and of the variation of self-induction. A very interesting feature is that the self-induction under these circumstances is not a wattless electromotive force, but that there exists an energy component of self-induction. Thus this paper brings experimental facts for a feature to which I drew attention some time ago, that under such circumstances an alternator without field excitation can yield electric power by the energy component of self-induction or can run as a synchronous motor, the energy component of self-induction yielding mechanical power. I may remark that besides this self-induction discussed here, the main phenomenon taking place under load in an alternator is armature reaction; that is the magnetic action of the armature currents on the magnetic circuit, and under circumstances a fluctuation of the magnetic reluctance of the field. In modern alternators usually armature reaction preponderates over the self-induction of the armature to such an extent that very frequently the armature reaction is three or four times larger than the effect due to the armature self-induction. Mostly, however, the self-induction is a very important factor also, and is dealt with very completely here.

MR. GANO S. DUNN:—It is not quite appropriate to the paper, but I have been interested in making measurements of the self-induction of direct current armatures, and if Prof. Goldsborough has made any observations on the self-induction of a coil in the neutral space and under a pole-piece, it would be very interesting if he would state what he finds the relative self-inductions to be.

PROF. GOLDSBOROUGH:—I have not made any determinations of the character of the variation of the inductance of separate coils of direct current machines, but I think you will find that the variation of self-induction as shown in this paper dealing with the Brush machine, is practically the same. Since the teeth on this machine are very large, they produce an effect simply magnified over and above what it would be in the ordinary direct current machine. If you move two teeth of an armature from under the pole tips into the neutral space, you will find there will be a variation in the coefficient of self-induction of the coil between them, that is practically the same as that shown in this paper. The curve of self-induction would be a little flatter at the top in such a machine, than in the machine I experimented on. In all probability, supposing this to be one pole of our machine and this another (illustrating), the curve of variable self-induction of a coil would be a very flat-topped curve for points immediately under

the pole-faces and would become small again as the coil passed between the poles.

MR. DUNN:—As I followed the paper this morning, the coefficients of self-induction were measured separately and without the influence of the mutual induction of the other coil.

PROF. GOLDSBOROUGH:—That is true.

MR. DUNN:—Does this coefficient that you speak of include the mutual induction of the other rings of the armature?

PROF. GOLDSBOROUGH:—No.

MR. DUNN:—My experience is that that very greatly modifies self-induction.

PROF. GOLDSBOROUGH:—It does.

MR. DUNN:—Therefore the results which were obtained with a single coil could not be expected to suffice for a coil under the actual conditions of direct current commutation.

PROF. GOLDSBOROUGH:—That is true. The machine which my paper discusses, gives the characteristic variation between the current and the electromotive force appearing at the brushes shown in Plate 17, where only one pair of the armature coils is active. On the other hand, this same machine operated with all the armature coils connected in series, develops about 500 instead of 190 volts, and develops a characteristic which bends over a little bit more and then goes straight down, so that you have perfect regulation for constant current between about 300 volts and zero volts. With all the coils connected in series and to collector rings, this machine can be short-circuited while carrying its maximum load, and the needle of the electro-dynamometer registering the current (which will not exceed 10 amperes) will not vary at all. The self-induction of the armature is increased approximately as the square of the number of the conductors as you take in the mutual induction of the adjacent coils. Just as you said, the mutual induction modifies the action of the machine considerably.

THE CHAIRMAN:—I do not think there is any great difference in the self-induction of a single coil and the self-induction of the coil as a part of the whole armature, including the mutual induction with neighboring coils, provided the conditions are such as would be the case in a continuous current machine under commutation. It would be different indeed if the current reversed simultaneously in a number of coils in series.

There is, however, a very essential difference in the self-induction of the commutation of a continuous current machine and that given here. If you take a continuous current armature and measure the self-induction of a coil by the method of reversals or by any other means, say by sending alternating currents through it, you find the value of self-induction larger than it is under commutation as a direct current machine, due to the very great difference of frequency. The frequency of commutation of direct current machines usually averages 400 to 800 cycles. For these frequencies the field structure which is in the magnetic circuit of

the commutated coil, is practically short-circuited. There is practically no commutating flux passing through the field, but all the flux passes through the air gap across the top of the slot. Therefore the real self-induction of commutation is less than the self-induction measured at low frequencies where the magnetic flux changes in the whole magnetic structure of the machine.

MR. DUNN:—The extremely high frequency in the direct current machine undoubtedly affects the coefficients of self-induction, although I have never measured it to bring out the point that you just made; but I do not see how the mutual induction can be so small a factor. I look upon the conditions as these: An iron core with two windings, one of which is the main part of the armature winding, connected with the constant potential circuit, and the other a short-circuited coil under the act of commutation. When the current reverses in the single coil the effect of mutual induction will be to send current through the rest of the winding out into the mains. Did I understand you to say that the mutual induction had a very small effect?

THE CHAIRMAN:—In an ordinary direct current armature, with closed circuit drum or ring winding, if two coils commute simultaneously or even in succession, and these coils lie in the same slot, thus having the same magnetic circuit of self-induction, then there is a more marked mutual inductive effect, which may be either helpful for commutation, or more commonly objectionable. If, however, not more than one coil at a time commutates in the same slot, I do not think there can be much mutual induction.

MR. DUNN:—Let us suppose that on the armature there are thirty coils and one is undergoing commutation. The resistance of the others would be twenty-nine times the resistance of the coil under commutation, but if the coefficient of mutual induction were high enough, a current as large as twenty per cent. even of the current in the commutated coil might flow.

THE CHAIRMAN:—Yes; but these coils are not individual coils closed upon themselves, but are in series, and are spreading over the whole surface of the armature in such a way that in a part of the series turns the E. M. F. of mutual induction is equal but opposite to that in the other part, so that the resultant effect is zero.

MR. DUNN:—I look upon them as being in series with each other, not over the whole of the armature, but over the two halves of the armature in multiple, and in series with the generating dynamo. The path of the current generated by mutual induction would be up one-half of the armature into the main, through the generating dynamo into the left half of the armature, and not in a complete circle around the armature.

THE CHAIRMAN:—I do not think you will find much, because in a symmetrical magnetic structure the resultant effect of mutual induction must necessarily be zero, and thus only due to a lack of symmetry, mutual inductive E. M. F.'s could be produced in the

armature outside of the commutated coil, but since you have the whole resistance of the generator in circuit, I cannot see that you can get a noticeable effect; I never noticed any.

MR. DUNN:—I understood you to say that you had measured it. My question was as to what the values were. I have not measured it but I have always thought it was large.

THE CHAIRMAN:—I have never measured it accurately. The only measurements I made were approximate. I measured the self-induction of the continuous current machine under commutation with ordinary alternating current frequencies, and then with frequencies varying from 500 to 1000 cycles per second and found very much lower values in the latter case, which brings me to the opinion that the lack of responsivity of the magnetic circuit reduces self-induction.

The following paper on "Electric Metering from the Station Standpoint" was then read by Mr. Caryl D. Haskins:

[See p. 265 June and July issues.]

DISCUSSION.

THE CHAIRMAN :—[Vice-President Steinmetz.] The discussion of Mr. Haskins' paper is now in order and I call upon Dr. Bell to open the discussion.

DR. LOUIS BELL :—The engineering point of great interest in connection with the subject of meters just at the present time, seems to me to be the problem of metering such loads as one strikes with motors, and especially with fan motors on the alternating system, and induction motors generally. The metering of the direct current as compared with the metering of the alternating current under these circumstances is a straightforward, simple problem, which gives no trouble either to the engineer or the station master. There may be trouble with direct current meters, but they are nothing compared with what are met in trying to measure in a just and reasonable way the volt-amperes energy delivered to the induction motors. As these modern plants are becoming more and more common, the central station man and the meter man may have to unite to find some way out of the difficulty. The trouble is right here: That if you apply an ordinary integrating watt-meter to a circuit having a highly inductive load, while you get paid for the energy delivered, you do not get paid for the capacity of the machine used up. It is a perfectly easy matter to run beyond the capacity of your station on a motor load, while your actual receipts as indicated by an integrating watt-meter would be far below the capacity of the station. Of course, the immediate impulse is to say—use a recording ammeter. But the recording ammeter, as Mr. Haskins has pointed out, scares the average customer. Then, on the other hand, the recording ammeter in many cases punishes too much. You do not care in handling such things as fan motors for a small increment of current due to their power factor; relatively it is a very large increment of current, but absolutely its effect on the station is not very great, because the amount of each motor is small, and the number is generally not sufficient. If you were to charge under these circumstances for current and not for energy, you would run up a bill against the fan motor which would apparently be out of all reason. Fan motors take current generally at relatively the same current as electric lights; that is to say, there is no special rate laid for them. On the other hand, when you come to have induction motors, particularly those which have bad power factors, and their number is great—when you come to these motors the matter gets more serious. You cannot afford to charge simply for the energy delivered, if you have any regard either for the capacity of your station or for the kind of service you are likely to give on mixed loads. Some way must be devised of charging not only for the watts delivered, but for the amount of reserve capacity which has to be set apart for running the motor in question. It is not uncom-

mon, and it is a very unhappy experience to find plants intended for the operation of induction motors simply at the end of their capacity for current, long before the output capacity of the motors has been reached. Those are the cases where the meter man is going to have a hard time. It is a difficult thing to adjudicate, and one which will have to be settled by continual conference between the station man and the meter man. My opinion is that the watt-meter is not sufficient for a case of the kind. You would be compelled to put the rate for the kilowatt hour so high, in order to get fairly paid under those circumstances, that the ordinary customer would kick. It would seem unreasonably high as compared with other motors, and it would be unreasonably high. It seems to me that some method ought to be devised akin to one that has recently been discussed, that is, a two-rate system which charges a basic price for maximum amperes, plus a watt-meter price for energy delivered. A scheme of that kind either carried out by arranging a fixed basic price per month, plus watt-meter price, or the plan which has been tried abroad of charging a certain rate for capacity, a certain fixed price plus a regular price. Either of those plans, either a fixed contract or an automatic plan having a meter to take care of the watt-meter, and some way of automatically taking account of the maximum current—some one of these plans I say must come into use under the circumstances, because neither the straight, ordinary watt-meter, nor a current meter, will fill the bill properly for these cases. If you charge for watts, pure and simple, you scare the customer. If you charge by amperes, you scare him even more completely and thoroughly, and as far as I can see the only satisfactory outcome for both parties is going to be in settling on some sort of double arrangement for the particular kind of service. I speak of it with special interest because that kind of service is growing every day, and it is the only example where metering does not seem to fit the case as well as it should. In passing—I believe as regards the character of the meters that we are going to have, that the biggest improvements, so far as the ordinary customer is concerned, speaking purely from the customer's standpoint, are the meters which I think will come into use before long which read directly so-and-so many watt-hours recorded in figures on the dial. Anybody who has ever tried to read an ordinary dial meter, not being familiar with it, will realize the position of the ordinary customer when he sees this queer thing spinning around on the wall, running up he knows not what against him. He looks at it as something uncanny, and he believes if Solomon lived in these days he would say: "Go to the meter thou sluggard."

DR. PERRINE:—I want to call especial attention to what Mr. Haskins says about what is to be considered as the range of voltampere accuracy of a meter. That we should not look for starting ability at extremely low load, and accuracy at full or

maximum load, but that meters should be installed as small as possible, which would give accuracy on overloads; a fair accuracy on overloads and very great accuracy on a small *ordinary* load; for I think that this is a principle that has been too little insisted upon, in general central station engineering practice, not simply in the question of meters but in the choice of engines, boilers, dynamos, transformers and every piece of apparatus about a station. Of course our engineers are working up to that more and more; but nevertheless it is a point that cannot be insisted upon too strongly, that real and true efficiency in the station is to be obtained by working our units always at a comparatively high load and high efficiency, installing apparatus which will stand overloads so that they will give high efficiency at the ordinary load at the station. This I have found in very many instances singularly neglected in central station practice, and we see its effect in the report of the National Electric Light Association. I have not seen their last report of coal economy, but that which was issued about a year and a half ago, shows that the best central stations in this country were using about three pounds of coal per horse power hour, and that the average throughout the country was not very much better than about five pounds, and some of the good stations run up to eight or even more pounds of coal. This, in the face of the fact that the dynamos are all of high efficiency; that in the electric central stations there are the best engines that the skill of the country can afford, and that in no one of those stations would machinery be accepted which would not give on test a better efficiency than two pounds of coal per horse power hour. But test means, test at full load, whereas running means running at a quarter or less than a quarter. Now it seems to me that in the case of all of our machines, not simply in the case of meters, but in the case of meters as well as in the case of other machinery, the machinery should be chosen where the full load is below the maximum load at which it is capable of running, and that in consequence, in place of machinery running at normally low loads with low efficiencies, the lightest loads should still be loads sufficiently great to give good efficiency. I was very glad to see that Mr. Haskins so well recognized this point and I think that its clear understanding and enforcement is one of the things that engineers need in order to bring up the general efficiency of their work.

PROF. OWENS:—I would like to ask Mr. Haskins if he considers that a meter which is correct on light steady load and also on full steady load would be accurate on rapidly varying loads?

MR. HASKINS:—Replying first to Prof. Owens' question, my experience has commonly been that a meter that will give accuracy at light load, and will give accuracy at high loads, will almost invariably be a good meter on intermittent loads.

PROF. OWENS:—How do you determine its accuracy on rapidly varying loads?

MR. HASKINS:—The method I have generally used for determining accuracy on varying loads has been to use a master meter, arranging two of the meters exactly alike and in multiple, but in series with a third or master meter, using a special form of switch between the master meter and the other two meters, so that the load could be rapidly switched through one or through the other or in multiple between the two, never off both of them. Then having achieved such an arrangement as that, I have hours at a time kept a boy or some one moving that switch, changing the load from half load to no load, from half load to full load, always passing through one or both of the two quantities under test and under all circumstances passing through the master meter, the load being constant on the master meter. I think that is perhaps the most accurate method of determination. I think I may say that my average experience while using this method has been less than one per cent. error, commonly within one-half of one per cent. It is a method I have used repeatedly and I have found it very satisfactory.

Referring to what Dr. Bell has said, I agree heartily with him and I think we can look far enough into the future to see that another form of two-rate meter is going to be called for, a meter whose rate of recording shall be dependent on the power factor. I think I am safe in predicting that when the demand comes, we can promise a meter which will automatically vary its rate with the variation of the power factor; this I think will be the simplest way of solving the problem. The use of the ampere-meter not only has the effect of discouraging the customer, as Dr. Bell says, but of discouraging one's own business. Referring to Dr. Bell's other suggestion that the general way of reading meters to-day is trying, I would say that I think the time has come when we can afford to have the customer read the dial easily. I think that within a short a time direct reading device will be procurable, but expense is the limiting factor in all these things. Such a dial as that is inherently more expensive, and people do not want to pay more for their devices.

PROF. OWENS:—Would not the addition of the so called Wright-Demand meter to the circuit containing a watt-meter solve this question of variable power factors?

MR. HASKINS:—Yes, I think it would. I think that perhaps is one of the best applications of the Wright-Demand system. The only trouble with the Wright-Demand system is that it does not segregate idle from active currents. If we had a combined load of light and power it might be conceivable that the maximum current was due to legitimate energy. I think that the Wright-Demand system would solve it admirably except for this consideration, which perhaps is not very important, but not so simply as a meter which would automatically and within itself change the rate, supposing such a meter were available. It is not available to-day, but it does not look like an impossible problem. I want

to thank Dr. Perrine for his endorsement of my views. As to the question of capacity I think that the great mistake that everybody has made, more particularly in metering and probably in our other devices also, is to demand laboratory instruments for commercial use. They want a meter that will give phenomenal accuracy on phenomenally low loads, ignoring commercial conditions. Meters twice too large are frequently put in, so that when the output increased they would be big enough, and yet phenomenal accuracy on light loads was demanded from those same meters, accuracy which injured the meter and made the meter too sensitive. It was like putting a laboratory instrument into common use.

MR. H. C. SPAULDING:—The remarks which have been made about the desirability of meters which allow direct reading by the customer and which should be cheap and practically free from liability to tampering, all had a bearing on a system which I have sometimes felt could be used to great advantage on ordinary power work, especially where the motors are in small units, as a modification of the old contract system where the representative of the company would squint at the pulleys and belts and do a little figuring in his note book and then make a contract for about 300 per cent. profit over the actual estimated cost of current. Those days have gone by, fortunately for some; but it would seem to me that on small motors and to a certain extent larger ones, it would be perfectly feasible to have a register which should so record that the motor could be charged for simply by the hour. That has been tried in some cases and I understand with very satisfactory results. The central station man can probably get more money for his current than he would with a watt-meter. The customer on the other hand knows that every hour that he uses his motor, it costs him just so much, this register being read at the end of the month precisely as on any ordinary meter, and he pays for so many hours use of the motor, the rate being made on what can be very reasonably judged as the average probable current consumption which it is fair to get at for an hour, but rather doubtful when you come to multiply that by 300 days in the year.

MR. A. E. CHILDS:—I would like to ask Mr. Haskins a few questions with regard to this slot meter which he has brought forward, as to whether he has any figures as to its use in this country and as to whether it has been successfully and satisfactorily used in Europe. The size and weight and cost are questions I would like to hear about too.

MR. HASKINS:—Replying to the last speaker, I have not any absolute figures on American practice. I think that American practice is very new, and what figures are available have been kept for the use of the initiators of the system. The system has been very widely used in London and has spread, I believe, to some four or five other large cities in Great Britain. I believe continental practice is somewhat slower and is behind that of

Great Britain. In a gas meter I understand that the extra cost is about 30 per cent. of the whole. I would add that the rate charge is something more than 30 per cent. higher. The weight of the gas meter, and it is almost entirely limited as yet, to gas practice, is not materially increased. While I have not any positive figures I think I would be safe in saying that the weight is not increased more than about 15 per cent., it as much. The method of operation varies in the different devices. The whole thing might be compared to a chewing gum machine such as one sees on the elevated railroad platforms. It is common to have a slot for coins and some kind of an actuating movement, either in the shape of a slight push rod or crank which does the mechanical work, and most of these devices are deficient in that they commonly permit the deposit of only one coin at a time. You have got to use up the value of that coin before you can deposit any more.

Referring to what Mr Spaulding said I think there may be some applications for what he mentioned. I believe that such a device was offered in the market for some time about two years ago. It was made in the west. It is better than the contract system, but it is not metering. Not *a propos* of what Mr. Spaulding said, though it is suggested to my mind by it, I want to tell a story. I had some connection some time ago with a young man who was engaged in meter testing. He had previously appealed to me for a ready method of testing motors in connection with meters, and I supposed that I had given him one, but he passed out of the employ of the firm with whom he was, and they kept his note-books. One day they brought me his note-book remarking that they saw noted under my name a method of testing meters with motors and wanted to know if it was right. It read like this—reminding me of what Mr. Spaulding said about squinting at a shaft—"Estimate the horse power of work being done by the motor, and test the meter by the common formula. If the results agree within 10 per cent., all right, if not, estimate the motor again."

THE CHAIRMAN (Mr. Steinmetz):—Gentlemen, I fully agree with Dr Bell that it is very desirable to have some means of charging for the wattless currents in alternating current circuits. It is not fair to charge a customer by ammeter for all the wattless current he uses, especially as you would not keep any customers in this way. It is not fair, because this current does not represent power to the customer or to the central station. However, it is not fair either to charge by watt-meter, because you then do not charge for those wattless currents which do represent power as loss in lines and transformers and wasted station capacity and thus interest charge. Therefore it would be desirable to charge for a part of the wattless currents, a part sufficiently large to take care of the losses due to resistance of lines and transformers and generator capacity. It is very easy to do that with the

recording watt-meter. The only thing necessary is to replace a part of the non inductive resistance of the potential coil by reactance. Assume that we insert in the armature or potential circuit of the recording watt-meter an inductive reactance equal to 20% of its impedance, reducing the resistance correspondingly, so that the meter reads correctly at non-inductive load. On a highly inductive load, of 20% power factor or so—as the current of an induction motor running light—the meter will read just twice the power or will read about one quarter of the wattless current. That is probably a very fair charge. You may make it anything you want by varying the self-induction. Such meters are in operation, but there is one difficulty with them, as you can see, not on the electric side of it indeed, but you cannot supply such meters except on special request, because if you supply such a meter, which is an entirely fair and proper meter to use, then somebody tests it under highly inductive load and finds the meter 100 per cent. wrong. The next thing you hear is, it appears in some paper, that the meter is entirely wrong and unreliable on inductive load. Thus you can only build such meters on special request. Now it is necessary to take some steps like this, especially where the motors are supplied by the customer; otherwise the customer buys some cheap kind of induction motor with a horribly low power factor, or at least installs a motor by far too large for the work to do, and runs it very light most of the time—that is at poor power factor—causing wattless current of excessive value to run all this time over the circuit without being charged for but using up line and station capacity.

There is one point in the paper on the wave-shape which is very interesting and where something more can be said. The meter should be correct for different wave shapes. For not only with different machines but even with the same machine on a different load, the wave shape of the circuit may change, not if you have lights in circuit only, but if you put in any inductive device, say a fan motor or reactive coil. The current wave consumed by the fan motor essentially differs from the wave of the electromotive force, and consequently there are current meters which read wrong in such cases and cannot give accurate reading. On a fan motor load some current meters read low, and some one told me, in all apparent truth, that if he had several lights running on a meter and leaves the lights on and puts a fan motor in besides, the meter slows down. I don't know whether that is not exaggerated, but there is decidedly a lag in the meter to read fan motor current. More vicious probably than the fan motor current is the current taken by the high frequency sets to produce X-rays, because thereby the secondary current, of the condenser discharge, is oscillating, and the primary current is decidedly affected thereby and broken up, so that it is quite likely a very large current may flow and the meter not take any notice of it. The watt-meter is much less liable to this difficulty. The only

error in a watt-meter may be due to very rapid oscillations which may not be recorded, but which are not very likely to occur.

MR. HASKINS:—In regard to what Mr. Steinmetz said in connection with the slowing down of meters by throwing on additional load, I would say that I have met such meters and tested them. Out of the west there came a story that a certain kind of meter (which by the way is not now obtainable), was in one town commonly reversed and backed up daily by a customer who ran it on his lights normally, and who, when he was not running his lights, put a special form of choking coil onto a lamp socket, thus backing the meter up. He generally backed it up long enough to nearly cancel what it made running ahead. I should scarcely care to absolutely vouch for this report however.

[Recess.]

EVENING SESSION—TUESDAY, JULY 27TH, 8.15 P. M.

The meeting was called to order by President Crocker at 8.15 P. M.

THE PRESIDENT:—Before proceeding with the business of the evening, I have to announce that to-morrow morning, after the first paper, Prof. Owens would like to present an invitation to the INSTITUTE to come to the Trans-Mississippi Exposition next year to hold the General Meeting, and I will also make an informal report to the INSTITUTE as to the adoption of the "National Electric Code." The first paper on this evening's programme is "A Historical Sketch of the Fire Alarm Telegraph" by Mr. Adam Bosch.

SECRETARY POPE:—I wish to call attention to the fact that the original signal box referred to in Mr. Bosch's paper, also the second form which followed it, and the third, are on exhibition in the collection of Mr. Farmer's inventions.

(Mr. Bosch read his paper, which is printed in the TRANSACTIONS at page 299 of the June and July issue.)

DISCUSSION.

THE PRESIDENT:—This paper is an interesting example of one of the important functions which this body should perform, and that is the putting on record, in complete and permanent form, of the development of various branches of our profession. This particular subject is interesting as being one of the early successful applications of electricity, and also one with which Prof. Farmer's name is associated. I know in my own work that it is often a matter not only of great convenience, but almost of necessity to have various subjects on record and in form sufficiently complete to be worth looking up, and particularly to have them in our own TRANSACTIONS. There is no place where they can be more available for reference than there. I think that the paper therefore is to be specially commended as putting this subject on record. If there are any questions or discussion upon it they are in order.

DR. GEO. F. BARKER:—I do not remember the precise date at which the magneto was introduced for fire alarm purposes, but I recollect very distinctly seeing the magneto which was driven by water power, and which was used in the fire alarm telegraph in Boston. The fact impressed itself very distinctly on my mind by a little episode I witnessed in the office one day. One of the operators was exhibiting the instrument to some of his friends. There were ladies in the party. He was running the machine very slowly and amusing the ladies by inducing them to take hold of the electrodes, and get a pretty decided shock. Meanwhile there was some construction going on above, and a bricklayer was carrying mortar and stuff from the top on a hod down to the foot of the stairs. He thought he would like to try it, and after the experiments were shown to the ladies, the operator put the whole force of the water on and drove the magneto up to its full capacity. In the meanwhile the hod carrier had been up with his hod and with another load of material he came down, and seeing that the instrument was entirely unoccupied he seized hold of the electrodes with both hands with very great vigor. The consequence was that the bricklayer, hod and all went on the ground. I am sure that that man if he were present could give us the precise date at which the magneto was introduced.

MISS FARMER:—May I tell you just a little incident I have heard my father relate with a great deal of pleasure. At the time of the exhibition of the car he had some other experiments. Among them he put a five dollar gold piece in a basin of water that was charged, which he offered to any man who would take it out. A big brawny fellow came there and took it out quickly, and father found that one of the wires was not connected up. The man had the five dollar gold piece. Father asked him to put it back again and told him he would put another one with it if he would. The man refused. He held on to his five dollar

gold piece. There was some excitement and one after another offered to put more to it until I think they got up somewhere in the vicinity of \$40 which they offered the man. He said no, that it cost him more than a month's work, and he kept his five dollar gold piece.

THE PRESIDENT:—If there are no further remarks on this subject, we will pass on to the next paper on the programme "Electric Traction, Notes on the Application of Electric Motive Power to Railway Service, with illustrations from the Practice of the Metropolitan Elevated Road of Chicago," by M. H. Gerry, Jr., of Chicago. In the absence of the author, the paper will be read by the Secretary.

[See June and July issue p. 275.]

DISCUSSION.

THE PRESIDENT:—Gentlemen, this paper is a most timely one and given to us in truly professional form. I think we can consider ourselves as owing a special debt of gratitude to Mr. Gerry. It is the very kind of paper that those of us who have to do with getting papers are anxious to secure.

DR. PERRINE:—I have recently been talking with Mr. Sprague about this problem of electric railroading, and on account of some of his ideas I regret exceedingly that he is not here to-night, and would like to speak on some of the points to which he has called my attention. In the first place, Mr. Sprague holds that the simple question of cheapness would not be one which would entitle a heavy road, such as an elevated road, to substitute electricity as its motive power. He makes the statement, which seems to me to be rather extreme—nevertheless we understand that he generally knows what he is talking about—that if on the elevated railroads of New York a system of power were applied which would lead to the hauling of one more passenger, per car, per station, in each train, the added revenue so acquired would amount to more than saving one-half the coal bill as at present used and, starting from this premise, what is demanded from the application of electricity to steam railroading is not so much cheapness as increased facilities. He attaches even more importance to the rapid acceleration of trains than is attached to it in this paper, where it is shown that rapid acceleration reduces the total amount of energy required to move between two stops a certain distance apart, and in order to accomplish this Mr. Sprague is now attempting to control from one end of the train a series of motors all located on the separate cars; so that not only do we have the track adhesion of our locomotive car, but with any loaded train we have the track adhesion which may be given by the entire weight of the train, and particularly on grades and curves this facilitates the rapid handling of trains. It seems to me that too much importance cannot be attached to this question of the rapidity of acceleration, for rapid acceleration means not simply the reduction in the amount of power necessary to move a train, but it means what is of still greater importance, the facility of getting trains out of the way and permitting new trains to come in. In other words it means the handling of the whole service on shorter headway, and in consequence, a greater possible number of trains, the handling of a definite amount of traffic with a less concentrated load on the structure, and also a more satisfactory service which will induce increased travel. This line of argument seems to me to be of very great importance. As I said, I am exceedingly sorry that Mr. Sprague is not here to-night to give us his ideas in his enthusiastic way, but this much I gathered, and coming together with this paper, it shows that the two engineers are thinking a great deal along the same lines which adds to the interest of the conclusions.

THE PRESIDENT:—I might add to what Dr. Perrine has said, that Mr. Sprague mentioned to me another advantage of the system which he advocates, and that is that you could maintain the same headway all through the day by simply reducing the length of trains for periods of light load, and he thought that would be a great factor in retaining the trade; that is to say, during the middle of the day the trains are run at long intervals; many persons get into the habit of using other means of transportation, whereas if you could keep up the same headway the public would become accustomed to using the elevated roads at all times. I believe that has been found to be one difficulty—the long intervals in the middle of the day impress people unfavorably in regard to the elevated roads, and he brought that out as one of the strong points of motor cars, which enable the trains to be made up of any number of cars and easily increased or decreased. That system certainly seems to possess many advantages.

THE SECRETARY:—The question of headway is of considerable importance in retaining the traffic, as many of us know from personal experience. One of the objections always raised to suburban traffic is, that one has to be governed by a time table, while on the Manhattan Elevated Railway one gets into the habit of starting when he is ready and waiting for a train. The difficulty of infrequent trains also occurs at night, at least after the theatres close. I suppose it is expected that everyone should be at home by that time. One goes to a station expecting to catch a train, and will have to wait perhaps fifteen minutes. That is rather objectionable when it gets to be twelve or one o'clock at night and the passenger is going out of town and has to make a train connection. Furthermore the management is not very particular about informing people as to the exact headway under which trains are run, and the consequence is that one is in entire ignorance as to when a train may be along, as no proper announcements are made that the trains are to be changed from a ten minute headway to a fifteen minute headway or whatever it may be. And that leads up to the manner in which they endeavor to accommodate their light traffic by taking off trains, and of course the more trains they take off the lighter the traffic becomes, because in New York City the elevated is paralleled by the cable road which at night is practically as quick and the cars more frequent.

PROF. THOMSON:—I happen to know of this suggestion of Mr. Sprague and there is no question whatever but that the advantages pointed out exist. There is just one remark to be made however in that connection, that complexity is introduced by having the cars all equipped with motors, and the difficulties of making connections from car to car are greatly increased of course in coupling; so that it may become a serious question as to whether we are not paying too much for some of these advantages; whether

in other words we ought to extend the complexity so far as has been proposed, to obtain the advantages which follow, or whether there is not some other way out of it. Each car, as I understand it, would have to be possessed of a controller system of its own, and that controller manipulated by some automatic mechanism from the head of the train. Of course this means a good deal of change of mechanism, some of which may get out of order and a few stoppages might be of more serious import to the success of the road than the disadvantages of having a simpler mechanism which did not fulfil so many conditions.

MR. DUNN:—I believe Mr. Sprague expects that even if the mechanism on one car or two cars does get out of order that the others will carry it right along, and he hopes by that means to reduce very largely that chance.

MR. C. P. STEINMETZ:—The problem of elevated railroad service is becoming exceedingly important at present. This is a city railroad service of high speed with very frequent stops, at intervals from 1500 to 3000 feet.

Some very interesting theoretical investigations of this problem have been made by one of my assistants, Mr. A. H. Armstrong, and will be published in due time, and since I had occasion to find the results checked by practical experience, I shall give a few of the conclusions, which by the way are brought out quite prominently by this paper also.

The elevated railroad service differs from all other railroading by the feature that the motors never run at full speed, but that the whole trip practically consists in acceleration and retardation. That is, the distance between stops is usually so short that before the train has reached the full speed of which the motors are capable, the brakes have to be applied for the stop at the next station. Thus the speed curve of such a railroad trip is as shown in the solid line in Fig. 20. This curve represents a run of 2000 feet in 70 sec. or an average speed of 19.5 miles per hour. As accelerating efforts are used, 100 lbs. per ton, as friction 10 lbs. per ton, and 150 lbs. per ton train weight as braking effort.

As seen, the train accelerates for 31 sec., reaching a maximum speed of 30.5 miles per hour. Then the power is turned off and the train runs on its momentum, that is coasting, until 52½ sec. after the start the brakes have to be applied and the train brought to a standstill for the next stop. By changing the amount of acceleration and retardation the speed curve can be varied to a considerable extent. Four such curves are shown in Fig. 21.

As seen, the lowest acceleration at which the distance can be covered in schedule time is 87 lbs. per ton. With this value, acceleration has to be maintained for 46.5 seconds and immediately after the brakes have to be applied, that is the range of coasting has entirely disappeared and the maximum speed reached by the train is 38.2 miles. The more rapid the acceleration is, the shorter time is needed for accelerating, and the more time

available for coasting and at the same time the maximum speed reached by the train decreases to 30.5 miles per hour at 100 lbs. acceleration, 27.2 miles per hour at 150 lbs. acceleration, and 24.8 miles per hour in the theoretical case of infinite acceleration, that is if the train could be brought up to maximum speed instantly.

One very interesting feature regarding the total amount of energy consumed by the train during the trip is brought out by these curves. With the change of the rate of acceleration, the maximum speed of the train changes from 24.8 miles per hour at infinitely rapid acceleration to 38.2 miles per hour at the lowest permissible acceleration, that is in the proportion of 1 to 1.54, and consequently the kinetic energy of the train in the proportion of about 1 to 2.4, and the energy put into the train for acceleration in a still somewhat larger proportion, since at the lower acceleration, the time of acceleration and thus the friction loss during this time is greater.

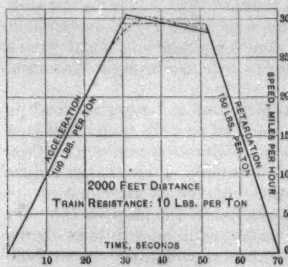


FIG. 20

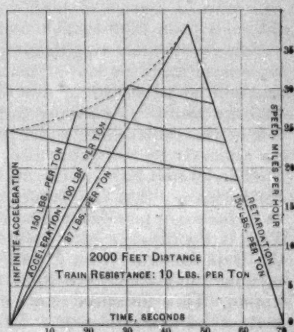


FIG. 21

Since the power put into the train up to the point of maximum speed represents the total energy required to cover the distance in schedule time, it follows that by varying the rate of acceleration the energy consumed by the train can be varied in the proportion of 1 to more than 2.4, or in other words with the slowest possible acceleration more than $2\frac{1}{2}$ times (allowing for friction during acceleration) as much energy is required as would be necessary at infinitely rapid acceleration, or in other words the more rapid the acceleration of the train the less energy is required to cover the distance in schedule time,—but at the same time the larger must be the current and power put into the motors during the time they are in operation, etc.

A lesser maximum speed than necessary during a run consisting of acceleration, coasting and retardation, can be reached with a given rate of acceleration and retardation by a run consisting of acceleration, running at constant speed and retardation.

The efficiency of the latter run, however, is considerably lower than in the former, as seen by comparing the two speed time curves a, b, c, d , and a, b', c', d in Fig. 22. At the same schedule time the areas of the two speed curves a, b, c, d , and a, b', c', d are equal. With the speed curve a, b, c, d , consisting of acceleration, running at constant speed and retardation, the total power put into the train is the acceleration up to speed b, c plus the power consumed by friction from a to f .

With the speed curve a, b', c', d , consisting of acceleration, coasting, and retardation, the total amount of power put into the train is that giving the train momentum at speed $b' c'$ plus the power consumed by friction during the time from a to e' , that is the time during which the motor is in circuit. Thus in the first case the power input is less by acceleration from b to b' , but more by the friction loss from e' to f . This friction loss, however produces retardation from b' to g and thus is equal to the acceleration from h to b' (where $h g$ is parallel to $a d$). Hence in the case of

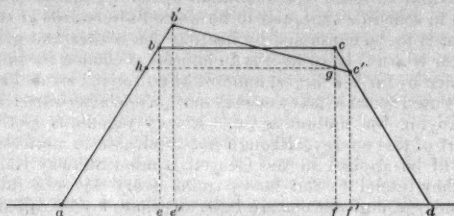


FIG. 22

running at constant speed, the power input exceeds that of the case of coasting with the power shut off, by the power consumed in the acceleration from h to b .

In another way this can be seen thus. The total power input during run is equal to that consumed in the brake during retardation, plus that consumed by friction. The power consumed by the friction is the same in any case. The power consumed by the brakes, however, is less in the case of coasting in which the brakes are applied at a lower speed, than in the case of running at constant speed, and consequently the economy of operation is higher in the first case.

Thus it is theoretically uneconomical to leave the motor in circuit after the period of acceleration is passed, and run at constant speed. In practice the run at constant speed is still much inferior due to the low efficiency of a motor built for high acceleration and running at speed, that is at very light load, and it is therefore preferable to accelerate and then coast until the brakes have to be applied.

A slight gain can be reached by dropping the acceleration off

slightly near the maximum speed, in running "on the motor curve" as shown in dotted line in Fig. 20. During the whole part of acceleration at constant torque, a greater or lesser part of the voltage has to be consumed in the rheostat, since without rheostat or equivalent means, the series motor cannot maintain constant torque at constant voltage but varying speed. This loss in the rheostat can be reduced partly by having the rheostat cut out completely somewhat before maximum speed is reached, and accelerate during the last part of acceleration on the motor without rheostat, that is with an accelerating effort decreasing with the speed. The permissible range of acceleration on the motor curve, that is without rheostat, is very short, however, since the torque drops off quite rapidly with increasing speed, and in practical operation sufficient margin must be left in the motors to carry a somewhat larger load, as at times of unusually heavy traffic, or to "make up time."

Thus the faster the acceleration the less energy has to be put into the train to reach the maximum speed needed to cover the distance in schedule time, and to be wasted afterwards in retardation, that is to be consumed by heating the brakes and grinding up the car wheels. Here then is an enormous chance for improvement, since by far the largest amount of power in elevated service is merely used to set a heavy mass in motion and afterwards wasted by destroying the motion again. A very ingenious method to use a part of this energy, although not applicable to most elevated roads, will be applied in the Central London Subway Railroad, where they intend to run heavy trains every $2\frac{1}{2}$ or 2 minutes. Their underground stations are built on what may be called hills, that is the road-bed is not on a level but rises to the station, so that when a train leaves the station it runs down a fairly heavy grade and gravity is used to accelerate the train, and in approaching the next station the up grade towards the station is used to retard the train, hereby less electrical acceleration and less retardation by brakes is needed, and motors as well as brakes greatly relieved, and it is estimated that in this manner about $\frac{1}{3}$ of the power will be saved.

Referring now to the ability of exceedingly fast accelerations by a very great subdivision of motors. Undoubtedly a considerable advantage is gained by making as many axles driving as possible. I doubt, however, whether it is necessary to have all axles driving. In elevated service the limit of permissible acceleration is probably not the adhesion of all the axles but the comfort of the passengers, and before you strike the limit of adhesion of even half the axles or less you probably reach the point where acceleration is so rapid as to be too uncomfortable to be permissible.

DR. PERRINE:—The two first statements of Mr. Steinmetz are liable to confused interpretation, and I think that the statements of the paper are also liable to the same interpretation. Taking the second first, as being the simpler, Mr. Steinmetz says that

acceleration as now practiced in electric railroading or in street railroading in general is as rapid as comfort will allow. I think by examining Fig. 4 page 281 you will see that it is not so, because you can stand just as rapid acceleration as retardation. Whereas the total retardation is in about 25 seconds, the total acceleration is in something over 60 seconds, which shows that we have a possibility, still within comfort, of a great deal more rapid acceleration than at present. The second thing he said was that the paper showed that the more rapid the acceleration the less was the power. This apparently contradicts his statement that the history of running a train between stations was simply acceleration and retardation. Thus this statement should be qualified to avoid misunderstanding. His meaning obviously was that in elevated railway service the motors never reach the constant speed at which they could run without rheostat, but that before this time the acceleration has to cease, and the speed be maintained constant by a rheostat, or the motor cut off and the train run on its momentum, that is retarded by the train friction, until the brakes are applied. There should be a time between acceleration and retardation by the brakes, as shown also in the curves given by Mr. Steinmetz, and the value of the rapid acceleration consists almost entirely in the fact that with the same time between stations we can actually bring our train up to speed, and remove the condition that the whole history of the train between stations is acceleration and braking.

MR. STEINMETZ:—Referring to Figs. 3 and 4 of the paper, while the total amount of time used for acceleration is considerably greater than that of retardation, you see that the first part of the acceleration curve is practically as steep as the retardation curve, thus this rapid initial rate should have been kept up during the whole acceleration and considerable power would have been saved.

I fully agree with Dr. Perrine that the most efficient way is to accelerate as rapidly as permissible until you have reached sufficient speed to carry you to the next station in schedule time, then coast until it is time to apply brakes in approaching the station. It was this very condition of economy which I tried to explain in my former statement as superior to the method of leaving the motors in circuit and accelerating until the brakes have to be applied for the next station.

PROF. THOMSON:—The desirable thing to do it seems to me, would be first to get energy into the train rapidly. Your prime object is to get an average speed which will consume a certain time between stations. If therefore, you can have your acceleration rapid and then coast the whole of the rest of the distance, of course, that means the best possible condition, because the energy put into the train is represented simply by the consumption up to the end of acceleration. Now if, on the other hand, you simply reverse the diagram, getting the same average speed, you acceler-

ate far more slowly: you consume the same amount of energy during that time: you store it up in the train and you now waste it all at the same rate you put it in before, by braking. Thus you have the same average speed maintained, and the same time consumed: but in one case a very rapid acceleration and coasting to the other station: whereas, in the other case the acceleration is very slow and a sudden braking. But of course in practice that may not fit the conditions. It may require too high speed to be maintained or too high an acceleration, more than can be obtained, so that the next best thing we can do is to get as high speed as is permissible at first, run along and come out at the end so that we have to use as little braking as possible: possibly putting on power again in case the speed fails to be maintained as is needed. Of course, the system must be flexible, and we do not ever hope to meet the theoretical conditions, but we must approach them as near as practice will allow. The point is, to determine what is the theoretical condition, and then see how nearly we can come to it in actual practice.

THE PRESIDENT:—It seems to me that the problem from a mechanical point of view is an extremely simple one. The fact that the author considers paradoxical, is simply this; that if you had an infinitely rapid rate of acceleration to a given speed and then an infinite rate of retardation as suggested by Mr. Steinmetz, and leaving out the question of stops, you would get the most economical condition, and hence the kinetic energy put into the train would be a minimum.

The following paper on "The Cost of Steam Power, by Mr. H. A. Foster," was read at the morning session, July 28th, by the Secretary, in the absence of the author:

[See page 315 June and July issue.]

DISCUSSION.

THE PRESIDENT:—This subject is one upon which we are very dependent, and it is as Mr. Foster says, one that you can get all sorts of figures for. They are chiefly based upon what people think they are doing, and the paper is particularly valuable as being intended to give the facts as they actually are. In my own experience, I have heard of cases where power was supposed to be generated at a very low figure, but as a matter of fact, it was costing a great deal. Mr. Foster has devoted a great deal of time and intelligent effort to reaching the real results. The paper is open for discussion.

DR. PERRINE:—I would like to further emphasize one fact, to which Mr. Foster calls attention and to which he apparently attaches a good deal of importance, namely, that of the results of the plant No. 17. In spite of the fact that it was one of the best equipped plants mechanically, and used expensive coal, the cost of horse power was but little less than obtained with another plant using a poorer quality of coal; and in this is involved a question of the method of testing fuel in order to arrive at a definite conclusion as to what coal should be used in any particular plant. At one time—and I think it still exists somewhat—there was a tendency to throw doubt on the value of calorimeter tests of coals. The mechanical engineers as a rule say that this is not the best test of coal, but that the best test of any particular coal is by firing under the boilers, and seeing what result can be obtained. That is, of course, nothing more than the arriving at the result that the fireman can handle a particular coal better than he can handle another coal, but that does not say at all whether better economy might not be obtained by discharging that fireman and getting a new man, or by introducing mechanical stokers, or some means of using a different quality of coal.

As regards the question of the heat units contained in a pound of coal, I think there is far less difference between the poorest coals ordinarily in the market and the best coals. This I know was brought out in a paper, I think before the British Institution of Electrical Engineers—at any rate, before one of the British societies, on the coast of steam power. I cannot recall at the present time the date of the paper or even the author, but I remember that in the discussion, in which engineers had given the cost of operating a number of different electric plants, one engineer gave an example of his own plant in which he was using a cheap grade of coal, and by which he claimed that he was obtaining very high efficiency. The statement was made, that he was using a particular quality of slack, giving not more than about 9,000 heat units per pound of coal, and as it was about one-half the best that can be obtained, there was quite a margin of loss of efficiency, which might be accounted for by the coal. He had not made any calorimeter tests, but arrived at this value of the heat units in the method that a great many engineers had arrived

at it, by guesswork. It happened that the particular quality of coal used by this engineer had been tested by the author of the paper, and in place of being low in heat units, it was reasonably high. Instead of being 9,000 heat units, it was 13 to 14,000, and in place of the plant being economical on the basis of the coal used, it was not economical on the basis of the coal used; and this shows the effect of that class of rough and ready tests. Most of our economy of electrical machinery has been reached through the fact that electrical engineers are not content with guessing or assuming the efficiency of their plants, but actual tests of efficiency are made of a scientific character, and when these results are obtained, electricians have sufficient faith in their results to rely upon them, and the introduction of this same scientific spirit into the question of coal handling is one that has done a great deal in this country towards decreasing the cost of steam power, and as the methods of the scientist are extended, it will do a great deal more. The fact is, in the neighborhood of Chicago most of the mechanical engineers have finally ascertained that of the coals they can get there, the lowest priced coal is in general the coal which gives the cheapest steam power. But that does not say that in any particular plant the lowest price coal necessarily gives the cheapest steam power, because if we have a plant designed for anthracite coal, we cannot economically burn bituminous slack on the grate bars; but if at any particular locality bituminous slack is the cheapest coal to buy, then the most economical plant, as a rule, will be the plant which is built and adapted for that low priced coal. While Mr. Foster does not insist on this fact particularly, nevertheless his experiments with this plant No. 17 show that the choice of coal gives a tremendous basis for a change in the price of the steam power. In this particular plant, No 17, a cheaper coal might give a lower efficiency, but, nevertheless, had the same care been used in designing the furnaces as was apparently used in the rest of the plant, it seems from the results of neighboring plants that a lower steam cost would have been obtained by the use of a cheaper coal, for otherwise there is apparently no result from the care with which the general machinery of the plant has been chosen. In all of this I mean to insist that we are necessarily to apply and rely upon the scientific methods of tests in every part of a plant in which we use our electrical machinery, and the rough and ready test of the every day mechanic is no more to be relied upon in the choice of fuel than it would be relied upon in the choice of a dynamo machine.

PROF. B. F. THOMAS:—I would refer the members to the results of a very careful study of the questions involved in different brands of coal, the methods of firing boilers in which they are used, etc., to work recently done by Prof. N. W. Lord, of the Ohio State University, Columbus, communicated lately to the American Institute of Mining Engineers and to be published,

in their Transactions. That, I believe, is the most complete and thorough test that has been so far undertaken. Prof. Lord has gone into the chemical constitution of the coals, their heating values by calorimeter tests and the practical tests under boilers of various classes, methods of firing, new temperatures and new gas compositions, and so on, and has made an exceedingly complete and protracted study of those questions. I think perhaps some who are interested in the coal question side of the cost of power will find information there of interest and value.

THE SECRETARY:—One of the interesting facts in connection with the great development of steam power is that of the changes in prices of coal and the quantities of each size brought to market, due to the demand that has grown up for smaller sizes through their coming into use gradually on account of their cheapness. Perhaps a case that will be known to most of you is that of the difference between ordinary stove coal and chestnut coal. Chestnut coal was formerly cheaper at retail than stove coal, but in use it was found that for many reasons chestnut was more desirable, and so gradually the price was raised until the retail price of chestnut is now the same as that of stove coal. Then again, there has been a lot of coal piled up in culm heaps that formerly was considered good for nothing. Pea coal was brought into market in 1867, and in 1876 buckwheat coal was introduced. There is also rice coal, although the latter is used for fuel only at the mines, and that coal has recently had a market price of its own; whereas, years ago it could not be given away. We all know that there are certain goods and valuables that bear transportation better than others, and on account of high freights I would like to ask Dr. Perrine, about the kind of coal that is used in California. We know that they use a certain amount of anthracite. That has to be brought from long distances and we can all well understand that there are certain grades of coal which it is economical to use only near the mines, and where there is no cost for transportation. But if you have to carry that coal a great way, of course the freight on it is as much as it would be on a better class of coal. Consequently, it appears to me that the practice in different localities would show that it is only the best coal that can be transported and used economically.

As a matter of record it might be well to quote here a table prepared by Wm. McClave, of Scranton, and included in a paper read by him at Philadelphia, January 9th, 1895, before the "Anthracite Coal Operators Association." He takes the best bituminous or anthracite as a standard.

TABLE OF RELATIVE VALUES BY PERCENTAGE.

		To equal Anth. Egg, add to each ton.
Bituminous Coal, good quality, ...	100%	
" Slack, ...	90%	11 1%
Anthracite Steamboat, ...	95%	5.3%
" Broken, ...	97%	3.1%
" Egg, ...	100%	
" Stove, ...	100%	
" Chestnut, ...	100%	
" Pea, well cleaned and of good quality	95%	5.3%
" Pea, mixed with bone and slate, ...	90%	11.1%
" Buckwheat No. 1, good quality, ...	93%	7.5%
" " No. 2, ...	85%	17.6%
" " No. 3, ...	83%	20.5%
" Culm No. 1, mixed with 20% soft coal slack good quality, ...	83%	20.5%
" Culm No. 2, mixed with 20% soft coal slack, good quality, ...	77%	29.9%
" Culm No. 1 (alone), good quality, ...	75%	33 1%
" " No. 2 (alone), ...	70%	42.9%

It can hardly be expected that such figures can be exact under the varying conditions, but they were obtained in actual practice.

DR. PERRINE:—That is largely true, at the same time, the practice in steaming in California is very bad, and it is largely on account of the fact that the coal dealers are the only steam makers. The coal is purchased in California by the electric railroads and the electric power plants on the basis of the production of pounds of steam per hour. The coal is entirely paid for by the thousand pounds of dry steam produced. The coal dealer has the option of using his own firemen or using the firemen belonging to the company he is supplying. Of course in either case the company supplied pays the wages of the men, but the coal dealer may take entire charge of the fire room and hire the employees, or he may simply have a representative there. The best coal that we get out there, the coal which has the highest calorific power, is Welsh anthracite, which is obtained by the largest dealers at about \$6.00 per ton. I paid \$11.00 a ton for it, and I can get a lower grade of bituminous for \$10.50. But the largest dealers can obtain a lower grade of bituminous coal at about \$4.50 a ton, but no coal could be had in California for less than about \$4.50 a ton by the largest dealers, and those are people who charter their own ships and bring in coal by the shipload. The most of our bituminous coal comes from Australia and our anthracite coal comes from Wales. There is very little Pennsylvania anthracite used. That has to come overland and costs from \$11.00 to \$12.00 per ton. Some people who are very particular and want good hard coal buy it. One would expect that these men would work out thoroughly the best possible grates and the best possible methods of handling coal, but the greatest advance they have made consists simply in bringing from the boiler a steam jet to serve as a blower to the fire. That serves not only as a blower, but gives a flame to this Welsh anthracite coal which it does not

possess without that agency. Only recently have some few of the younger engineers made careful tests with change of grate-bars and the use of the cheaper coals, and up to the present time they have not obtained very good results because the manufacturers are slow to see that the experiments may result in any great advantage. They buy steam, not coal, and they are reasonably satisfied with the price that the steam costs them, so they are not willing to put much money in change of furnaces, especially as the coal is controlled by a very close combination, and they do not know but that if they should generally introduce bituminous coal of a low grade, that then the bituminous coal of a low grade would be held at the same price as the anthracite of a high grade. Furthermore there is hesitancy on account of the fact that the low grade bituminous coal comes from Alaska and Washington, with which there are comparatively no return freights, so that while we can bring a little coal down from that country cheaply, if we should have to carry a great amount of coal it would mean freights only in one direction and the freight rates would run up. The coal is brought in the wheat vessels. A good deal of our flour goes to Australia and a great deal of our wheat goes to England. The whole stock of California coal comes into the State during the autumn, when the wheat ships are arriving for their fall cargoes, and these combined elements have held the question of consumption at one point for a great many years, and as I say have directed steam users to the economic value of the very best coal that we can get, which is really a better coal than many coals you get here on the Atlantic coast. I do not think there is any, except possibly the gas coal that you have here in the east, that has as high calorific power as the Welsh coal.

PROF. R. B. OWENS:—I feel that one of the best directions in which electrical engineers can direct their efforts is in inducing the owners of small electric light and power plants, to keep a continuous coal and water record. Of course it is done in our large plants and in a few isolated ones. For instance, in the Board of Trade plant in Chicago, a continuous record of water and coal is kept and averages are obtainable which it is impossible to get in two or three ten-hour tests. Coals of all kinds are tried, and the number of pounds of water evaporated, under standard conditions, per dollar's worth of coal, is obtained, which is really the only proper way to express the value of coal, or any other fuel used for steam generation. In some of our large lighting plants, the cost of coal is now, I believe, less than the lamp breakage. It has come to be one of the smallest items of expense; whereas, in smaller lighting plants in interior towns, it is more than one-third the total expense of operation. In these latter, therefore, the coal pile must be watched with especial care, but as I said before, very little of value is obtained by short runs. For instance, it is possible to get from New River coal, an evaporation of 13 pounds, from and at 212, under proper conditions of firing. Captain Jones of Chicago, formerly

of the Navy, last year made a test, using New River coal, of a well-known water-tube boiler fitted with a good automatic stoker, and after three or four preliminary trials, actually got 13 pounds—13.1 pounds evaporation, I think it was, from and at 212. No such record, however, has ever been made under the ordinary conditions of continuous service, or, is likely to be.

A MEMBER:—Is that per dollar's worth of coal?

PROF. OWENS:—No, per pound. It simply shows that short tests amount to very little. It is only when records are kept extending over months and perhaps years that anything of real value can be obtained.

MR. DOUGLASS BURNETT:—Before we change the subject, I wish to say that Mr. Foster has my own personal and hearty thanks for the great care that he has evidently bestowed in fishing out a lot of information which is certainly valuable—extremely valuable—and I think that one of the aims of the INSTITUTE should be to combine the science with the commercial side, and I believe that in this respect Mr. Foster's paper is perhaps the best paper that I have heard, because he combined what we are taught in our colleges, the work that we do in connection with our mechanical engineering, with the results obtained in commercial practice. I have encountered personally some of the difficulties that Mr. Foster has, and I know that when he is able to summarize 22 cases in which he can get accurately at the cost of power he has done a great deal of work. He has my personal thanks.

MR. W. H. RIPLEY:—In regard to what Prof. Owens says about the importance of keeping a close record, I noticed on Long Island a couple of years ago, where a plant was started, there was a large supply of coal on hand in the yards. The coal was of very good quality and high cost, and the plant was started on that coal. The engineer in charge was a man of a great deal of experience, and had studied a good deal besides his actual firing work. When the supply gave out, the directors of the company who were not very far-seeing people anyway and had all along been complaining of the price of coal, got a chance to buy a quantity of very much cheaper coal—that is what they looked at, the price of it; and this man had been keeping very careful records of the evaporation per pound every day, and he made a weekly summary and a monthly report to the company, and kept track of the percentage of ash. The percentage of ash in the coal they started with was, I believe, about $8\frac{1}{2}$ and with this cheaper coal the ash was over 13 per cent. After over two months of talk, presenting the matter to those directors who were mostly residents there, we finally convinced them that they were getting a smaller amount of steam, and that the increased amount of ash just about balanced the difference in price in the coals, but it took a long while, and I think that is an example of the difficulties that men in charge of steam plants have—that managers or purchasing agents look at the cost of the coal alone, and unless some very careful record is taken it

is almost impossible in some cases to make any improvements in the cost of the steam.

THE PRESIDENT:—If the discussion is over, there will be some announcements.

MISS FARMER:—I have a telegram which should have been received on Monday, which I will read:

MISS SARAH J. FARMER.

I regret exceedingly that recent absence abroad and urgent business engagements since my return, have prevented preparation of my intended paper on Electric Science, and to my great disappointment I am also denied the privilege of attending the meeting of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS at the noble memorial you have established to the memory of your distinguished father. I am however with you in spirit, and furthermore by a coincident which I hope may serve as an auspicious augury, this day, the Fiftieth Anniversary of the first exhibition at Dover by Moses G. Farmer of the movement of a car by electricity, has seen the inauguration and first public demonstration of electric railway carrying a new departure in electrical propulsion, as distinguished from locomotion, practically the logical consequence of the adoption of electricity for railroad work, a train of six electric cars having been successfully operated by a boy of nine years. The advancement of the electric railway in the decade since the installation of the Richmond railway is unparalleled, and nothing in the industrial world has had a more widespread and beneficent effect on the social and moral well-being of the community. In 1887 there were scarcely a dozen railways in existence. To-day there are 700 in the United States alone. Then there were barely 100 miles of track; now there are nearly 14,000. From 150 cars the equipment has grown to over 35,000, propelled by 52,000 motors. Prediction perhaps would be idle, but it would seem that we are entering on a new era of railway progress whose extent cannot well be limited, in which is concerned not only city but suburban and in a large measure interurban travel. Such will be electricity in all its characteristics.

FRANK J. SPRAGUE.

THE PRESIDENT:—I am sure we are all very much interested to hear this telegram read on this occasion. It is most unfortunate, however, that we do not have Mr. Sprague here in person, because his personality is particularly strong and interesting, but he shows some of it, I think, in his telegram.

I believe Prof. Owens has an invitation to present to the INSTITUTE. The INSTITUTE would be glad to hear it.

PROF. OWENS:—I have the pleasure of extending on behalf of the President and Board of Directors of the Trans-Mississippi Exposition, an invitation to the Institute to hold its next general meeting in Omaha. Every facility will be afforded for making the meeting a success. Probably in a great many years the west will not have such features of electrical interest as may be seen in Omaha next summer. I hope that some expression may be obtained from those present in this regard, for the sooner the place of meeting is decided upon, the easier it will be to make arrangements for a successful one.

MR. T. C. MARTIN:—Mr. President, I think most of us have listened to the invitation from Prof. Owens as a special Commissioner from the State of Nebraska with a good deal of interest and pleasure. It seems to me that after coming, as we have here, to the extreme eastern tip of the continent, it would at least be proper and in order that our next oscillation, as you might call it,

should carry us somewhere toward the centre of the country. We have been inclined in our meetings to linger eastward and it is certainly high time that once more we turn our faces to the west; and perhaps to render the contrast complete I do not know that we could seek anything in sharper difference from the sweet serenity and peace of our surroundings here than the bustle and hurlyburly of the west, as we shall find it, proceeding from Chicago and staying a few days at Omaha during that exposition. I think, moreover, that we, as a body, owe a little to the State that our friend represents. I had the pleasure of being in Lincoln last year and I was deeply impressed with the magnificence, the splendor, the regal generosity with which that state had endowed her educational facilities, and particularly the eagerness with which they were seeking there to give further scope and larger aim, and higher purpose, to the scientific and technical training of her young men. If we go west upon such an occasion as this we shall not only be doing something that Nebraska will deeply appreciate, but we shall be furthering the purpose which underlies the existence of the INSTITUTE itself. I would like, therefore, if it please you sir, to offer this resolution, which, I will say just by way of preamble, is not intended in anywise to commit us now. I do not think that that would be proper. I do not think it is perhaps within our province. The object of this resolution is simply to get from this meeting an expression of its feeling on the subject, and though perhaps this is not as large a meeting as it might be, we have rarely had a more representative gathering than at this time. Another fact I would like to mention is that we have a large number of members in the west; possibly the preponderance is west of the Alleghanies and certainly we do wish to extend our membership out west and the best way to extend it is to go west and go after the membership. My resolution is this:

RESOLVED that it is the sense of this meeting, that it is desirable to hold the next General Meeting of the INSTITUTE at Omaha and that the invitation, extended by Prof. Owens be favorably referred to the Council for further consideration and early action.

MR. DUNN:—Mr. President, I desire to second this motion. I think that a meeting in the west next year would be particularly appropriate. I think that the holding of it at Omaha at the time of the Trans-Mississippi Exposition would enable us to assemble a large gathering, and when Mr. Martin mentioned the hospitality that we should find in the west it brought to my mind pictures of how well our western friends would receive us, and do everything in their power to make us comfortable and to make our meeting a success.

MR. STEINMETZ:—I think this motion is a very timely one. Our INSTITUTE has in the last years extended so greatly westward that the centre of membership undoubtedly lies considerably further west than the average meeting place. Now we have so

many members in Chicago and San Francisco whom we have never seen and who possibly may come to Omaha, but who certainly will not come to us here, and since for years and years most of the meetings have been held here in the east and even in the far east, so that a meeting farther to the west would be very timely. I should heartily endorse this motion.

MR. C. W. PIKE:—It appears to me there are three very good reasons why we should go to Omaha. In the first place there has been a good deal of criticism of the management of the INSTITUTE, because it is considered to be a New York concern. I know of course that that criticism is by no means just. At the same time it exists. I think it would do a great deal to dissipate that, if we did get into the habit more extensively than we have in the past of holding our meetings outside of New York, more especially in the western states. In the second place I have no doubt that we should have a larger meeting in Omaha than perhaps any place that we could choose, because we should have the Trans-Mississippi Exposition for one thing, and because people in the west think nothing of traveling a few hundred miles. In the third place, Prof. Owens has invited us. I should therefore most heartily second this motion of Mr. Martin to refer this resolution favorably to the Council.

The resolution was carried unanimously.

THE PRESIDENT:—I will take this opportunity to make an informal statement for the information of the INSTITUTE. As a delegate appointed by the INSTITUTE to represent it at the National Conference on Standard Electric Rules called together to formulate and promulgate one uniform code of electric rules for the whole country, I have to report that the Conference has successfully accomplished its work and that the code is printed. This Conference was composed of the following bodies: The American Institute of Architects, THE AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, The American Society of Mechanical Engineers, The American Street Railway Association, Factory Mutual Fire Insurance Companies, National Association of Fire Engineers, National Board of Fire Underwriters, National Electric Light Association and the The Underwriters' National Electric Association; these being practically all of the bodies which have any direct or even indirect interest in this question, and it would therefore seem to be a particularly complete organization. In fact, I think it can be said to be one of the most complete and important aggregations of technical and commercial interests that was ever called together for a specific purpose of the kind, and I think it is a matter of congratulation that it was able to perform the work for which it was organized. I can also say for the information of the INSTITUTE that the influence of the INSTITUTE and the fact that it had taken this somewhat unusual step, had a most favorable effect in securing the success of this Conference. I am sure that if this INSTITUTE had held itself aloof, as it has in the past

in such matters, the Conference would not have been a success. I will report formally to the Council and strongly recommend the approval of this code by the Institute, and I feel warranted in making this recommendation in view of the fact that it has already been adopted by several of the influential bodies, in fact more than one-half of those represented in this list I have given to you.

The next paper on the list is on "Efficiency and Life of Carbons in Enclosed Arc Lamps," by W. H. Freedman, H. S. Burroughs and J. Rapaport. The authors are not present and as I am somewhat familiar with the paper I will read it and will call upon Vice-President Steinmetz to take the chair.

Mr. Steinmetz took the chair and Dr. Crocker presented the paper as follows:

[See page 361 August and September issue.]

DISCUSSION.

THE PRESIDENT:—I would like to ask Dr. Kennelly, who obtained results similar to these, if he has any explanation for them—that the distribution of the light of an enclosed arc lamp has the general form represented in Fig. 11.

DR. KENNELLY:—In regard to the peculiar wing-shaped curve of luminous intensity, when we made the measurements to which the President has just referred, we noticed, of course, that abnormal upper development, which is absent, I believe, in the case of the naked arc, and we assumed at the time that it was due to light reflected from the inner globe upwards to a zone above the positive carbon. In other words, had the inner globe not been there, the entire prominence would have been about 45-degrees below the horizontal plane through the arc; but with the globe in place, many of the rays so transmitted were thrown back again by reflection to an upper zone.

PROF. OWENS:—I would like to ask if the shape of that curve is altered by the peculiar form of the small enclosed globe?

THE PRESIDENT:—I might answer both the statements by saying that it was suspected that the light reflected from the inner globe was the cause of that peculiar distribution, but it was not possible to prove that fact, and the test made with different forms of globe, I believe, did not add anything to the knowledge. Undoubtedly that must be the cause; there would seem to be no other, and possibly changing the form of the inner globe considerably, would prove this to be the cause: but no conclusive test has been made, so far as I know.

MR. L. B. MARKS:—I have no doubt that the form of the small globe influences the shape of the curve. By using a globe that has a very marked curvature, instead of a long flat globe, you get a different candle-power curve; that kink in the curve is not very common in globes that vary in form from the usual shape. I presume that the writers of the paper did not find much difference in the various curves, because the small bulbs which they tested are all very much the same in shape.

I have been very much interested in some parts of this paper, and note on page 363 one of the lamps I designed and some tests made on it. I find that the authors obtained results which are very much the same in some cases as those of former experimenters. Indeed the work of Thomson, and Blondel, and Nichols, and some of my own researches seem to have brought out most of the general results that are given in this paper. I have not had time to examine it very carefully, but I think perhaps a few statements in regards to some of the matters that are presented may be of interest.

On page 366 we have a curve for the relation between voltage across the arc and length of arc. While it is true that this curve is of some value in representing the ratio

alluded to, still I hardly think that it would be safe to use a curve of this kind as a basis for any important measurements, because it seems to me that that curve is really one expression of the ratio between the voltage and length of arc of this particular lamp. This ratio varies very greatly in different lamps and in fact even in the same lamp. If you take two cored carbons, for instance, you will find that the ratio varies. You will also find that a figure such as eighty-three volts corresponding to the length of .023 of an inch, as given in Tables VI., may not apply. It may be 20 or 25 per cent. off. Again the ratio will naturally depend on the position of the arc in the bulb. The authors do not give the conditions under which this particular test was made, and without these conditions the tests lose much of their value. If the arc, for instance, is at the top of the bulb, the ratio will be quite different from what it would be if the arc were at the bottom of the bulb. The ratio naturally varies with the amount of air that enters the bulb. We will assume, for instance, that in this lamp which the authors tested, the bulb is air-tight at the time, and the arc is near the top. Then take a condition in which there is a small amount of air entering the bottom, as there is in some of these lamps I presume, and we will assume that the arc instead of being placed at the top is placed near the bottom,—I will venture to say that there will be a difference of perhaps 30 to 35 per cent. between the measurements thus obtained and in the values given in this table. I have made quite a number of tests and I find that it is very difficult to give any particular curve that will come near truly representing this ratio.

The regulation of the lamps is a matter which of course requires a great deal of time to ascertain with absolute certainty. The authors have gone to a great deal of trouble, apparently, to measure the lamps, and I am glad to find they have given us the results of tests which cover a period of months instead of a period of weeks, as is often done.

There is a very interesting point in the paper which the authors have not gone into very fully—perhaps one of the most interesting points in connection with the enclosed arc, and that is the voltage at which an enclosed arc can be safely and steadily run as compared with the voltage of the mains at which such an arc can be run. The nature of their tests is very much the same as those of Professor Thomson, which were recorded in a paper read by him at the convention of the National Electric Light Association at Niagara last month. Their results are however somewhat different than his, and I may say that the results of Professor Thomson, and those given in this paper, are quite different from my own. Take Blondel's measurements made in Paris and you find these are different, and some recent measurements of one of Dr. Nichol's students at Cornell are also different. We have five or six sets of measurements none of which coincide. It

would seem to me that it would be extremely valuable for some one to take up that particular part of the table and enlarge upon it. I have no doubt that a thorough study of this one point would bring out a very valuable series of figures of which, at the present time we have no exact knowledge at least as far as published data are concerned. I may say with regard to the remarks made by the authors of the paper, that while they claim that an arc cannot be run steadily below a certain voltage of the mains, which I believe they figure here about ninety-five or so, that this is hardly true, and will depend on conditions. I presume they took the commercial lamps in the market: they used the ordinary bulb, and so on; but if you vary these conditions, that is, change the diameter of your carbon and the amount of current, change the size and perhaps the shape of the bulb and the position of the arc in the bulb, and the nature of the gas checking devices and the quality of the carbon, etc. you will find probably that an arc may be run at a voltage which is very close to that of the mains under some conditions; so that the broad statement that an arc run on mains that are below ninety-five volts is unsteady, would hardly be true; it would depend on conditions, and although the authors, to be sure, confine their measurements to certain commercial lamps, still it is rather misleading to read their statement which apparently refers to all lamps of this type. Then again as the voltage of the mains goes down, it is advisable in order to run an enclosed arc steadily, to operate at slightly lower voltage of the arc. Now the authors have adhered to seventy-eight volts at the arc. They cut out resistance and they lower the voltage of the mains, yet they keep the figure seventy-eight volts at the arc. Under those conditions it was quite natural that the arc should be unsteady at a lower voltage of the mains, and I think that if they had reduced the voltage of the arc when they reduced the voltage of the mains they would have gotten entirely different results. So that the figures here apply to a very narrow set of conditions.

I will not touch on the mechanical points they refer to, although naturally a great deal might be said. I shall try to confine myself as closely as possible to purely technical points.

On page 371 at the lower part of the page, paragraph 3, the authors say:

"The size of the enclosing globe affects the life, it being less by a slight amount for a smaller globe. All the other conditions being the same, a lamp having a smaller globe will, during a given run, have its globe replenished with air from the outside oftener than if it had a larger globe."

I think that that statement is open to question. Anybody who has experimented with various sizes of enclosing bulbs will have found that there is quite a marked difference in favor of the smaller bulb; that is, with a smaller bulb, under ordinary conditions, the life per inch of carbon is greater. True enough,

we do not get the total life of 100 or 150 hours, because there is not sufficient carbon: but the life per inch of carbon consumed is ordinarily greater, in some cases considerably greater, with the smaller bulb. Naturally in changing the size of the bulb, in order to get the best results, it is necessary to change other conditions—to change the gas checking effects for instance whatever they may be. But I believe that generally speaking, the life of a carbon is increased, or the consumption of carbon per unit time is decreased with decrease in the size of the enclosing bulb.

On page 372 we have different forms of gas caps. The gas cap is quite well known now. A few years ago I gave it the name of "gascheck." All these forms of gas caps are the same in principle. They consist simply of some chamber device for holding part of the gas formed by the action of the arc, and in that way acting as a check against the ingress of air and egress of gas.

On page 373 there is a very interesting point which Dr. Crocker alluded to, I believe, as miraculous or important.

THE PRESIDENT:—Only peculiar.

MR. MARKS:—Well I admit that at first sight it does appear very peculiar. But the explanation of it seems to be quite simple. In most lamps, as is stated in the paper, the relative life of the positive and negative carbons respectively changes as the arc descends in the bulb. Now here is an apparent exception to the rule. The average ratio is about 1 to $2\frac{1}{2}$ or 3, I believe. My own measurements are a little different from that—perhaps 1 to 4; but they find a case 1 to 10 given on Table 9, page 375. That would be accounted for in several ways. If the voltage of the arc decreases, the ratio naturally increases, and at a certain voltage of the arc and a certain current, it is possible to increase the ratio so greatly as to approach infinity. That is, instead of having a ratio of 1 to 2 or 3, or 1 to 10, you may go up to 1 to 20 or 1 to 50, or more, depending on the voltage of the arc and the current passing at the time.

Regarding the photometric tests, I think we have spent a great deal of time in photometric measurements, and it seems to me that to a certain extent it is time wasted. With the enclosed arc, the character of the light, that is, the quality of the light is quite different from that of the standard adopted in order to make the measurements comparable with others, and I think in some cases the relative measurements of candle power would be meaningless. The authors state very truly that the candle power measurements are not to be relied upon, and they give their reasons. They refer to the difference in value between the standard source of illumination and the arc, and the very marked difference in the color of the light, and point out the incandescent lamp gives an orange light even when run at its normal voltage. They also refer to the personal error in comparing the lamps. I think perhaps more importance should be attached to this last point than is given here. Different experimenters

will get quite different results. In Ithaca we made measurements of the enclosed arcs, and we had ten watchers, and there was a difference of over 50 per cent in the maximum and minimum readings taken at the same angle. Of course this difference is enhanced by the "wandering" of the arc. I am glad to see tests given on the holophane globe. I think this is the first time that any measurements have been given of the relative value of the light with and without such a globe, that is with the enclosed arc lamp. I know that when this question was asked me abroad by Prof. Blondel I had to admit that we had not made any tests in this country.

On page 383 referring to the two bulbs, I hardly know how to account for the deposition of carbon. In case No. 2, it is noted that there is a deposition of carbon at the lower end of the bulb which the authors state was burned into the bulb. If the lamp were operating, as it does normally, it seems to me that this deposition should not occur. I do not know what lamp these bulbs belong to. In fact I do not know the key to the paper. But I have seen bulbs of this particular kind operate in service, and if I am not mistaken there are several thousand of them in New York City, and the bulb which I am referring to, certainly does not give the trouble alluded to here. We have had practically no complaints regarding the burning in of carbon or incrustation of carbon in the lower part of the bulb, and I think that if the authors had operated the lamp under different conditions, or perhaps normal conditions, they would have obtained different results. In other words, nothing but an abnormal condition would account for the incrustation of carbon. Regarding the deposition of brass or some metal in the upper part of the bulb, that is probably due to allowing the arc to play too high in the bulb, probably very close to the gas cap.

The conclusions drawn by the writers seem to cover almost all the advantages of the enclosed arc lamp. There is perhaps one point of some importance which they have neglected to allude to, and that is the ability to run these lamps singly on an incandescent circuit. They refer to that in the paper, but in summing up the advantages of the enclosed arc over the open arc lamp, they have accidentally omitted this point. In many cases, such as small stores, it was necessary formerly to run two lamps in series. Now it is possible to furnish a single lamp to meet the demand. I have a note here, stating that it is to be regretted that the lamps should have been numbered. It would have added very greatly to the paper, I think, to have stated the names of the lamps, giving us the key to the situation. I know that I for one could have discussed the paper impartially and perhaps thrown more light on some of the peculiarities which the authors found. However, I suppose that it was impossible to do this under the circumstances. That is all I have to say regarding the paper, except to add that I am very much pleased to see

that the authors have gone into the matter so thoroughly; even if they have not presented many new points they have at least chronicled facts.

MR. HOLLON C. SPAULDING:—I wish to add a word of appreciation to the paper on this subject, which has become of daily importance, and to bring out one or two points that I would like to get some more information on. Table No. 8, given on page 373 shows a condition of things rather different from what those of us who are principally on the commercial side have thought to be the fact. I would like to know whether these tests, A, B, C, D, and E, were made on one lamp, (suggesting also the desirability of knowing what lamp it was,) or whether the different tests were on different lamps; also referring to the statements on page 375 about the possibility of having a shellac coating on the upper carbon rod, and the reference again on page 383 to the carbon rods. It may not be out of place to call attention to the fact that one of the lamps has no carbon rod, and my own experience has been such that I feel very strongly inclined to add to the requisites of successful enclosed arc lamps on page 365, that they should have no carbon rod. Beyond that, and perhaps acknowledging the inadvisability of knowing which lamp was used for each test, it might perhaps be fair to know what carbons were used, or if different kinds were used.

THE PRESIDENT:—As to the first point I think that the tests of life on page 373 were on different lamps. In regard to the carbons, they were the ordinary carbons used by the different manufacturers, that is to say, the carbons that were recommended to go with the lamp and supplied with it.

MR. MARKS:—Probably "Electra" carbons.

MR. SPAULDING:—That would be so in the case of the Manhattan.

THE PRESIDENT:—I am glad that Mr. Marks was able to give us the benefit of his long experience in discussing this paper, because the paper was taken up from an entirely different standpoint. Absolutely disinterested parties simply took lamps as they found them, the standard lamps on the market, and gave them a pretty thorough, and certainly an impartial test, and it was not intended to go into the general theory of such lamps—the physics of it, you might say, but of the engineering problem to decide what these different lamps will do, and by parties who were not interested in any way in the different lamps. To give the actual names of the different lamps was not considered exactly proper, although it was stated that they were representative types. All the lamps were included, but individualizing was carefully avoided, and I think wisely under the circumstances. Any considerable variations in conditions were of course impossible because the lamps were simply taken as they were found, and used as recommended by their manufacturers which would seem to be the ordinary condition of practice. There is very little information

on this subject available, particularly from disinterested parties, and it was thought that the same information that was useful to us would be useful to others, and I might add that the investigation was originally taken up with the idea of determining for Columbia University whether it would use these lamps, and if so, which ones.

MR. STEINMETZ:—I have been very much interested in this paper, and in the discussion by Mr. Marks. There is however one point which I believe needs some further elucidation, since it may lead to mistakes. That is on page 382. The light efficiency is given there, but it does not state whether the power consumed by the arc alone has been considered or the power consumed by the whole lamp including the rheostat; furthermore whether the lamp was tested when new or after running some time and with a deposit formed on the globe.

The values given there, half a watt per candle, appear at the first view abnormally low—far beyond anything reasonable. But the explanation is that these are not the spherical candle powers. In open arc lamps, it is frequently the custom to measure "hemispherical" candle powers, considering the light thrown upwards as wasted—as may be quite proper—perhaps, where the arc is used for street lighting. The enclosed arc however finds its most useful field in indoor lighting, in competition with the incandescent lamp, and in this case the light, which is thrown upwards, is not lost, but reflected by the ceiling and to a large extent even more useful than the direct light, by giving a diffused illumination which is more valuable than direct illumination. Consequently it would only be proper to measure the spherical candle power, and then with two clear glass globes you would get one watt per candle and with one opal and one clear globe, 1.12 to 1.20 watts, and with two opal globes 1.8 to 2 watts per candle, which means in such case an efficiency fairly close to that of the incandescent lamp. Hence the enclosed arc lamp takes an intermediate position between the open arc and the incandescent lamp in its efficiency. The efficiency is lower than that of the open lamp and higher than that of the incandescent lamp. It shares with the incandescent lamp many valuable features, as the long life, absence of danger from the arc, etc., but it shares the other feature that the light decreases with the time, by the blackening of the globe. It has however the advantage of the incandescent lamp in the latter respect that the quality of light does not change; it remains the same light in color, but decreases in brightness; while in the incandescent lamp the light is yellow and reddish.

I do not think the conclusions drawn in the paper are quite complete, but they are rather one-sided, only the advantages being given. The open arc lamp is undoubtedly a very valuable piece of apparatus. But it has disadvantages, too.

As a conclusion I may then add as disadvantages of the en-

closed arc lamp—lower efficiency compared with the open arc, and decrease of light with the time of running.

It would be interesting also to compare it with the incandescent lamp, with which it competes in indoor lighting, and we find then, that the enclosed arc lamp has over the incandescent lamp the advantage of greater efficiency and constancy of the quality of light, but the disadvantage of being a little more complicated in its operation and not quite as handy, and besides, not allowing such extended subdivision of light, having necessarily larger units of light.

MR. CHARLES T. RITTENHOUSE:—On page 368 the authors bring out the point that a saving of about 13 per cent was obtained when using 100 volts compared with 115 volts. Mr. Marks has pointed out that it is not necessary under good conditions to use any external resistance at all.

MR. MARKS:—No, excuse me. I did not say that. I said the voltage might be lower.

MR. RITTENHOUSE:—I understood that the external resistance was not absolutely necessary. Granting that external resistance is necessary, I should like to know, when two lamps are operated in series, whether the gain would be greater than indicated by these results. That is to say, would one lamp tend to regulate another lamp, or is it necessary to use about the same resistance in each of them?

MR. MARKS:—Do you mean when two lamps are operated in series and on a 220 volt circuit?

MR. RITTENHOUSE:—Yes.

MR. MARKS:—Yes, there is a slight gain there, inasmuch as it is possible to operate the lamps at a higher voltage at the arc. As the voltage of the mains goes up it is possible to operate lamps in series as high as 100 volts across the arc, so that there would be a gain in that case.

MR. RITTENHOUSE:—I should like to ask whether it is necessary to have as much external resistance in series as when one lamp is used alone. That is to say, would the resistance in series of the two lamps be double that used in the one lamp.

MR. MARKS:—No; that would not follow.

CLOSING SESSION. WEDNESDAY, JULY 28th.

THE PRESIDENT:—We have assembled this afternoon in special session to read one paper from this morning's list, and the one assigned for this evening in order to avoid the necessity of holding an evening session. The paper to be considered now is on "Armature Reactions in a Rotary Transformer", by Professor Robert B. Owens, of Lincoln, Nebraska.

[See page 385 August and September issue.]

DISCUSSION.

THE PRESIDENT:—The paper gives the result of interesting experiments on this type of machine which is now coming into quite general use. It has some peculiar properties. One point that I think is interesting is the fact that the converter efficiency coincides with the motor efficiency, but is slightly greater at nearly full load. It would seem to me that it ought to be even higher than it is in respect to the motor efficiency.

PROF. OWENS:—The converter was not worked to its full capacity. The machine was only loaded to the capacity as a motor but not as a converter.

THE PRESIDENT:—If carried further, the converter efficiency would have arisen considerably above that of the motor.

PROF. GOLDSBOROUGH:—I have found in looking at Plate 6 of Prof. Owens' paper a most interesting set of curves. To me the problem of the rotary converter is a very attractive one, and this set of curves presents a great deal that is of interest, in view of the fact that it gives us the first series of curves ever published showing the variation of the distribution of the flux over the air-gap of an alternator for various instantaneous values of the armature current.¹

These curves illustrate very nicely the character of the armature reaction which takes place, and I think also give an insight to certain of the current reactions which take place in the armature conductors, and which I do not believe are mentioned in the paper. In a direct current motor-generator of similar character, that is in a machine having a double armature winding, and which, we will say, transforms current from 100 to 200 volts, the currents of the motor part flow in an opposite direction to the currents of the generator part of the machine. That is, providing the machine is of 100 per cent. efficiency, and that the resistance of the receiver circuit is negligible, the armature reaction of the motor part will be entirely annulled by that of the generator part, and there will be no distortion of the air-gap field due to the currents flowing in the armature. Now, as there is but one winding on the armature of a rotary converter, the alternating currents flow in the same winding, but in an opposite direction to the direct currents, and therefore the current density in the conductors of the armature must vary considerably from time to time. To indicate possibly a little more clearly the point which I wish to make, I will draw a figure here on the board. (Fig. 7.) Suppose we consider for a moment a rotary converter, which has an efficiency of from 60 to 65 per cent. This is a low efficiency for a rotary converter, but such a machine would produce an alternating current in the receiver circuit having a maximum value equal approximately to the value of the direct current supplied the ma-

1. In the spring of 1895 two of my students, Messrs. Crane and Reeves, made a determination of a similar set of curves, taking them from a six-pole single phase rotary converter.

chine. In other words, if (N) represents one pole of a bipolar machine, (S), the other, and o. r., its Gramine ring armature, we can take a base line A, to represent the zero line of current, and another line B, to represent the value of the direct current flowing into the armature at +. Then, supposing that we have simply a single phase secondary alternating current, which we can obtain from the bipolar direct current armature if it is tapped at two diametrically opposite points 1, 7, we can let the curve C, represent the instantaneous value of this current, and it will flow from the collector brush +. Now, with this machine, if I mis-

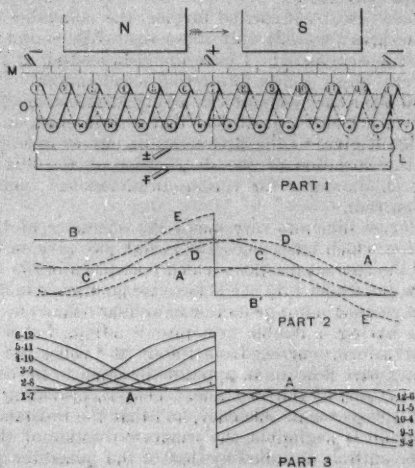


FIG. 7.

Keep the poles and curves stationary and move the conductors: then the current in any conductor in any position is determined by projecting it upon its current intensity curve of Part 3. The dots and crosses on the conductors show direction of direct current.

take not, with the brushes + and - resting on the commutator, M, as shown, when the alternating current rises to a maximum, which it does with the armature in the given position, the alternating current will be automatically drawn straight through from the direct current, and therefore at this instant of time there will be absolutely no current flowing through the conductors on the armature of the machine. This represents a particular case, and one which we could only have when we should design a machine with relatively low commercial efficiency. It could be brought about by increasing the iron losses to a considerable extent, or by

loading it mechanically by means of a pulley and belt, so that the electrical output of the machine would be low relatively to the input. Now the curves of Plate 6, I think, bear out this condition, in a somewhat more complex case; in one in fact in which the efficiency of the machine is somewhat higher than in the machine which I have assumed. To look at the matter however a little more closely before going on, I will consider that we have in Fig. 7, a section of the armature of the machine. We have here, we will say, a conductor 7, which connects with the commutator segment right under the + brush. To right and left of this we have conductors in this way, each one of which is connected with a commutator bar, and wound around the Gramme ring. We will consider that currents flowing toward us in the conductors on the upper surface are positive currents. Below the Gramme ring, we can draw the collector rings from which the alternating current is taken, and suppose, for instance, that I connect the upper alternating current collector ring to bar 7 of the armature by a wire, and the lower one to bar 1. Rotation may be considered in the direction of the arrow, for instance. Suppose we designate the conductor which is on the right side of the bar 7 that is connected to the collector ring, by the figure 8, and the conductor on the left side by the figure 6. The alternating current, which we have represented by the sinusoidal curve *c*, will have the same intensity in both conductor 8, and conductor 6, and we may consider now the combination of the alternating and the direct currents in the armature and the actual value of current intensity which results in the conductors at successive intervals of time. When conductor 8 is over on the left side of the centre line, for instance anywhere between position 1 and 7, it will carry a current which is equal to the value of the curve *B*, plus the ordinate of the curve *D*, corresponding to the assumed position of conductor 8, or to the ordinate of the curve *E*. This means that if we start from conductor 5, since at this point the alternating current in conductor 8 is zero, the value of the alternating current will rise up above the straight line *B*, until it reaches the central position at + in the centre; then suddenly, when the maximum value of the alternating current is the same as the direct current in the armature, the current will entirely disappear from all the armature conductors. It will begin to rise again in conductor 8, but in a negative direction as soon as conductor 8 passes under the + brush. Curve *EE'* is then a curve which shows the instantaneous values of the currents in conductor 8. Of course, in the case of conductor 9 an entirely different curve is required to represent the variations of the current in this conductor owing to its different position relatively to conductor 7. Now conductor 6 will have a current intensity curve which is entirely analogous to curve *EE'* except that it is reversed in position as shown in Fig. 7, Part 3. The current intensity curves of all the other conductors are also con-

sidered separately in Part 3. The curves show to some extent the character of the current changes and the current intensity modifications which takes place in the conductors on the surface of the armature.

Now referring to the curves of Plate 6, I think we can trace some of the points which I have mentioned in connection with this hypothetical machine very readily. For instance if you will note the position of curve 1 of Plate 6 you see that it is a curve which is high on the right hand side, and you will notice further that the curve 1 has a greater altitude on the right side than the other curves. It indicates therefore that at that instant of time the alternating current in the coils of the armature was zero and that therefore the distribution of the magnetism over the face of the air-gap is the same as that represented, or analogous to that represented in Plate 2, which shows the distribution of flux over the air-gap when the alternating current is zero; that is Plate 2 represents an exploration curve taken when there was no excitation at all of the alternating current circuit. In other words it represents a loaded exploration curve of the machine, time being counted from left to right. It is analogous to curve 1 of Plate 6, where, as determined by comparison with Fig. 4, and the statements made at the middle of page 391, time is counted from right to left. Now the alternating current, as soon as a movement takes place to either side of the zero position begins to rise. It rises until finally, at the position marked 6 or at the position marked 12, the alternating current reaction equalizes that of the direct current. At those points therefore you have just sufficient alternating current reaction to compensate for that of the direct current, and you get a perfectly symmetrical field in the machine. I would not say that at this point the currents flowing in all the armature conductors have a zero value, as the conditions are not such as to produce that result. Nevertheless it indicates a point where the reaction of the current flowing in the armature is lost for the moment. Now for points extending between curves 6 and 12 of the series of loops of Plate 6, there is a distortion of this curve in the opposite direction. That is, when the alternating current is zero you get curves that show the natural distribution of the flux which would occur in a simple direct current machine, then as the alternating current rises, the reaction of the direct currents is neutralized, and finally you get a condition shown at the middle of the series, in which the alternating current actually overbalances the direct current in the armature. That occurs for the reason, that the alternating current rises to some value that is greater than the direct current, and therefore when the armature reaction due to the alternating current, overbalances the armature reaction which is due to the direct current, you get an effect which is shown where the maximum instantaneous value of the curve of magnetism is changed in its position and runs over the other way, causing

a rise on the left hand side. Now I think we can trace the effect of the variation of the current still further in the efficiency curves of the machine. For instance, in Plate 10 you have the efficiency curve of the converter rising above the efficiency curve of the motor. In other words, the heating losses in the conductors are proportionally less when the machine is fully loaded and used as a rotary converter than when it is used as a direct current motor, for the reason that the current flowing in the armature is less, and that therefore the loss due to the current flowing in the armature is correspondingly smaller, and for this reason, largely, the efficiency of the converter rises for high loads. The losses due to the eddy currents and hysteresis would be practically the same, or even greater I think, in the case of the converter, owing to the fact that the armature reaction in this case, if anything, tends to increase rather than diminish the total field flux, and therefore any increase in efficiency is very largely due to the fact that the heating losses in the armature conductors are less, and also to the fact that there is no side pull on the shaft. There is less friction loss necessarily in the case of this rotary converter, than there is in the case of the motor.

MR. DUNN:—These curves presented by Professor Owens lead me to call attention to a physical fact which I think is not well known in electrical science, and that is the high resistance of certain forms of iron. These curves show a variation of the flux passing across the air gap with each impulse of the current, and the variation is sufficiently great, as can be seen, to cause a good deal of loss due to eddy currents.

The fact that I wish to bring out is that, while the resistivity of iron is ordinarily seven times that of copper, the resistivity of cast steel is about fifteen times that of copper or nearly double that of iron. The resistivity of cast iron is about 65 times that of copper or 4 or 5 times that of steel. This fact shows, why we do not get enormous losses in the case of rotary transformers which cause a variation in the field flux, as shown in this paper, the simple reason is that while there is change of electromotive force in the pole pieces, that change cannot produce eddy currents because of this abnormally high resistance. This resistance of 65 times the resistance of copper is something that makes the properties of the cast iron exactly like those of a different metal, and I can only explain it on the same ground that an extremely great increase in the resistance of copper is explained, when there are present some impurities in the way of phosphorus or sulphur. The reduction in the eddy currents that this causes is wonderful, and we have long been enjoying the advantages of that without knowing it. I see Professor Owen's machine is one in which a cast-iron field was used, and very likely the large variations here shown do not cause excessive loss on that account.

MR. C. P. STEINMETZ:—In large rotary converters a very con-

siderable loss of energy by local oscillation of the magnetic flux and consequent heating of the field pole pieces when solid is to be anticipated, and for this reason in such machines laminated field poles are ordinarily used.

Referring more particularly to Prof. Owens' paper, it is of great interest investigating what may be called the higher harmonics of armature reaction.

In the polyphase rotary converter the resultant armature reaction of the machine as direct current machine and as alternating current machine are equal but opposite, thus neutralizing each other and all that remains of armature reaction are effects of higher frequency which may be called higher harmonics of armature reaction. They are obviously present in alternating current generators and synchronous motors also, but they are overshadowed by the fundamental wave and thus less noticeable than in the rotary converter.

Besides these higher harmonics of armature reaction, further higher harmonics exist when the rotary converter is used to change from alternating to direct current, due to the difference in wave shape between the impressed alternating terminal voltage and the induced alternating counter E. M. F., which difference causes a current component to flow, consisting essentially of waves of higher frequency or higher harmonics.

The rotary converter has latterly become an eminently important piece of apparatus, by forming the connecting link between the alternating and the direct current. That is, giving a means to convert the alternating current received over long distance transmission lines into direct current for use.

In the rotary converter the induced direct current voltage and the induced alternating current voltage stand in a definite proportion.

In the single phase rotary converter their ratio is:

$$1 \div \frac{1}{\sqrt{2}}$$

Thus the ratio of the direct current and the energy component of alternating current corresponding thereto is:

$$1 \div \sqrt{2}$$

In a polyphase system we have to distinguish "star" voltage (in the three-phase system commonly called Y voltage), that is voltage between any terminal and the neutral point, or point of equal difference of potential from all phases, and "ring" voltage (in the three-phase system commonly called "delta" voltage), that is voltage between adjacent terminals of the system. In the same way we distinguish between star (or Y) current, that is current flowing from terminal to neutral point (this is the line current) and ring (or delta) current, that is current flowing from terminal to adjacent terminal.

The induced direct current E. M. F. and the induced star E. M. F. in the polyphase converter have the proportion:

$$1 \div \frac{1}{2\sqrt{2}}$$

The star E. M. F. and the ring E. M. F. of the n -phase system have the proportion:

$$1 \div 2 \sin \frac{\pi}{n}$$

Thus the induced direct current E. M. F. and the ring E. M. F. in the polyphase converter have the proportion:

$$1 \div \frac{\sin \frac{\pi}{n}}{\sqrt{2}}$$

Since the direct current power output and the alternating current power input, neglecting losses and phase displacement, are equal, we have;

The direct current and the alternating star current in the polyphase rotary converter have the ratio:

$$1 \div \frac{2\sqrt{2}}{n}$$

The direct current and the alternating ring current have the ratio:

$$1 \div \frac{\sqrt{2}}{n \sin \frac{\pi}{n}}$$

These current values refer to the energy component of alternating current corresponding to the direct current.

If a phase displacement exists between alternating E. M. F. and current, the wattless component of current has to be added by the parallelogram to above given values, and so has the current representing the losses in the converter, that is driving it as synchronous (or direct current) motor.

Derivation of these equations I have given in a lecture before Cornell University, published in the Sibley Journal, June, 1897. Vol. XI., No. 9.

The above given ratios of E. M. F.s refer to the induced E. M. F.s of the rotary converter. The ratio between the alternating terminal voltage and the direct current terminal voltage of the rotary converter differ more or less from the above given values, due to the drop of voltage in the resistance of the rotary converter and due to the difference of the shape of impressed alternating current wave and of counter E. M. F. wave; that is the maximum value of the alternating E. M. F. wave is equal to the

direct current E. M. F. plus or minus resistance drop. Thus the effective value differs more or less from the above given values, sometimes 10 to 15%, according to the ratio between maximum and effective value of the alternating E. M. F. wave, that is according to the wave shape. In the above given values sine waves were assumed.

In general in every armature coil of the rotary converter the direct current and the energy component of alternating current flow more or less in opposition to each other.

The direct current in the armature coil is really a rectangular alternating current as shown in Figs. 8, and 9, as C reversing successively in adjacent coils by their passage under the brush.

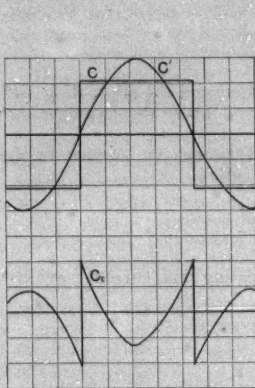


FIG. 8.

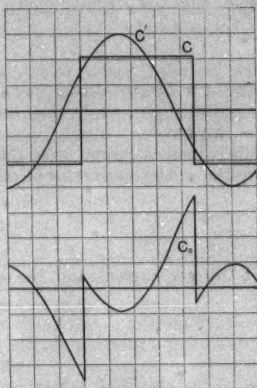


FIG. 9.

The alternating current C^1 is assumed as sine wave and is the same in all coils of the same phase, thus more or less displaced from the rectangular direct current wave.

The two currents are perfectly in opposition in a coil midway between two adjacent alternating leads, and in this coil the resultant current in the n -phase rotary converter is

$$C_o = \frac{C}{2} \left(\frac{4 \sin \varphi}{n \sin \frac{\pi}{n}} - 1 \right),$$

where

$$C = \text{direct current, and } \varphi = 2 \pi N t,$$

it is shown in Fig. 1 as C_o .

In an armature coil displaced by phase angle ω from middle position between alternating leads, Fig. 9., the resultant current is

$$C_o = \frac{C}{2} \left(\frac{4 \sin(\varphi - \omega)}{n \sin \frac{\pi}{n}} - 1 \right),$$

as shown in Fig. 9 as C_o .

Thus the effective value of current,

$$C_o = \frac{C}{2} \sqrt{\frac{8}{n^2 \sin^2 \frac{\pi}{n}} + 1 - \frac{16 \cos \omega}{n \pi \sin \frac{\pi}{n}}}.$$

Hence, since $\frac{C}{2}$ is the direct current in the armature coil, the relative heating effect of the resultant current in the rotary converter, and the direct current is:

$$\gamma \omega = \left(\frac{C_o}{\frac{C}{2}} \right)^2 = \frac{8}{n^2 \sin^2 \frac{\pi}{n}} + 1 - \frac{16 \cos \omega}{n \pi \sin \frac{\pi}{n}},$$

and integrating over the whole armature, it is:

$$I = \frac{8}{n^2 \sin^2 \frac{\pi}{n}} + 1 - \frac{16}{\pi^2},$$

the ratio of the $C^2 R$ in the rotary converter and the direct current machine of the same output.

In consequence hereof, regarding heating of the armature, the rotary converter can be rated at $\frac{I}{\sqrt{I}}$ of the output of the same machine as direct current machine. This is

.85 of the output of the machine as direct current machine in a single phase rotary converter,

1.34	in the three-phase rotary converter
1.64	“ quarter-phase “ “
1.96	“ six-phase “ “
2.31	“ infinite phase “ “

Coming now to the armature reaction of the rotary converter.

The armature reaction of the direct current C with m turns on the armature of a bipolar machine (in a multipolar machine C and m refer to one pair of poles) is,

$$F = \frac{m C}{2} \text{ avg } \cos \begin{matrix} +\frac{\pi}{2} \\ -\frac{\pi}{2} \end{matrix}$$

$$= \frac{m C}{\pi}$$

In the n -phase rotary converter the ampere turns per phase on the armature (per pair of poles) are:

$$\frac{m C_1}{n} = \frac{\sqrt{2} m C}{n^2 \sin \frac{\pi}{n}}$$

Where C_1 = alternating ring current and C the corresponding direct current.

Thus their resultant, since these ampere-turns are distributed over $1/n$ of the circumference:

$$\begin{aligned} f_1 &= \frac{m C}{n} \text{ avg } \cos \frac{+\pi}{n} \\ &= \frac{\sqrt{2} m C}{\pi n} \end{aligned}$$

and consequently the resultant armature reaction of all n phases:

$$\begin{aligned} F_1 &= \frac{n \sqrt{2}}{2} f_1 \quad *) \\ &= \frac{m C}{\pi} \end{aligned}$$

that is equal (but opposite) to that of the direct current. Therefore, while in a direct current or alternating current machine a considerable increase of field strength is required with increasing load to take care of the armature reaction, in a rotary converter practically no change of field strength with change of load is required, except where phase displacements are intentionally produced thereby.

The polarization of the armature due to the energy component of alternating current is in quadrature with the field M. M. F. Thus the armature reaction of the wattless component of alternating current is in phase with the field M. M. F. with a lagging, in opposition with a leading current in a machine converting from direct to alternating current. Hence, it is always magnetizing or demagnetizing, but never distorting.

In the polyphase rotary converter no distortion of the magnetic field exists, except that due to the small energy component of alternating (or direct) current driving the machine as motor, that is supplying the internal losses.

The resultant armature reaction due to the energy component of alternating current corresponding to the direct current is zero. Thus all that exists of armature reaction, is:

The small armature reaction due to the current driving the machine as motor, which is in quadrature or distorting, and of fundamental frequency.

* "Theory and Calculation of Alternating Current Phenomena," Steinmetz, page 854.

The armature reaction of the wattless component of alternating current, if such exists, which is magnetizing or demagnetizing and of fundamental frequency.

The higher harmonics of armature reaction investigated by Prof. Owens.

The higher harmonics of armature reaction produced by a difference of wave shape between impressed and counter E. M. F. when converting from alternating to direct current.

Either of the latter two reactions is oscillating.

In a single phase rotary converter the armature reaction of the alternating current is pulsating, thus the resultant armature reaction oscillating, thereby causing a sweeping flux, which is very objectionable for sparkless commutation.

The polyphase rotary converter is self-starting from rest, but in this case requires a very large current and gives a small torque, that is, is similar to the induction motor without resistance in the secondary circuit.

The polyphase rotary converter offers the most convenient means of controlling the voltage of an alternating current system by phase relation. By varying the relative proportion of impressed and counter E. M. F. by a change of the field excitation of the rotary converter, either by hand, or automatically by the combination of shunt and series field, leading or lagging currents can be produced at will, and thereby the self-induction of the circuit between generator and rotary converter made to raise or lower the voltage at the latter's terminals. Besides the rotary converter can be used to compensate for lagging currents of induction motors, etc.

The use of the rotary converter to change from direct to alternating current is somewhat dangerous, since any change in the alternating current system supplied by the rotary converter, which causes lagging currents to flow, will react demagnetizing upon the rotary converter field, and thereby cause the rotary converter to speed up in the same way as any direct current motor with reduced field strength speeds up. Therefore when used in this manner, means have to be provided to check automatically any undue acceleration caused by lagging alternating currents.

PROF. THOMSON:—There is just one consideration I would like to call attention to in the discussion of this subject and that is that the machine which is excited as a constant potential machine has virtually on its field a copper band which is of course in inductive relation to the mass of the field such as would tend to prevent variation in the total flux through the whole section, but of course would not prevent variation in flux relatively, the one portion of the pole piece to the other, and it would be of course an interesting matter to determine just how far this difference prevents the discovery of other variations or greater variations than those found.

THE PRESIDENT:—Are there any further remarks on this sub-

ject? If not I will ask Vice President Steinmetz to kindly take the Chair.

Mr. Steinmetz took the Chair.

THE CHAIRMAN:—The next paper in order is Professor Jackson's paper on "Electrical Cooking Apparatus." In Mr. Jackson's absence I will ask the Secretary to read the paper.

[See page 397 August and September issue.]

DISCUSSION.

PROF. THOMSON :—I am sorry that the author of the paper is not here to give us a little information that it seems to me ought to be had, especially in this country of pie. The cost of baking a pie is given, but the size of the pie is not mentioned. In the same way the cost of biscuits is said to be the same as that of pie. This looks a little suspicious, as biscuits are usually small. Whether for the purpose of comparison the size of the biscuits ought to be increased to that of the pie, or the size of the pie to be reduced to that of the biscuits, I do not know. As an interesting fact in this matter of cooking, I may mention that many years ago, I think about 1877, in a lecture, I showed as a novelty at that time,—dynamos were then a novelty—I showed the experiment of boiling an egg, and it was at that time quite a novelty. The experiment was performed by simply immersing a little coil of german silver wire. There was very little water, and in that operation you could bring it to a boiling point in a short space of time. I cooked the egg beautifully and ate it after the lecture.

PROF. OWENS :—The statement that electrical cooking apparatus is likely to help out the station load diagram, I think is an error. The breakfast cooking, and dinner cooking will certainly come about the time when the heavy morning and evening loads are on. I note that he has made no comparison with the gas stove which seems to be the favorite utensil for cooking in a small way. Not having had the experience with domestic cooking apparatus etc., that Prof. Jackson has, I am unable to criticise the figures he gives for the cooking of actual food material; but I conducted a long series of experiments a year ago to determine the efficiency, using water in every case as a means of absorbing the power, of various pieces of electric cooking apparatus, and I am sorry I have not the results with me, or the curves, but I will be glad to send a few of them in as a written communication.

THE SECRETARY :—I regret, Mr. Chairman, that this paper came to hand at such a late hour that I could not make any inquiries in regard to these matters. I am not very well posted on the subject of cooking by electricity, but my thoughts turned in the same direction as Prof. Thomson's, because, being a native of New England, and a long time sojourner in New York, I have had experience with different forms of pie and different sizes of loaves of bread, and I have seen loaves of bread baked in a dish-pan in camp. If you could bake one of those for six cents, it would perhaps be a reasonable cost. I was unable to get any further information, and this paper was printed after I left. I received it since I arrived here.

THE CHAIRMAN :—Is there any further discussion? If there is no further discussion, the programme of the June meeting, 1897,

is closed. But before the meeting adjourns, I think I express the sentiment of the INSTITUTE by expressing the heartfelt thanks of the INSTITUTE to the residents of Greenacre, and especially the ladies for their hospitality, and the very pleasant reception they have accorded to us. In the name of the INSTITUTE I thank them. (Applause).

The General Meeting then adjourned, *sine die*.



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